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AMERICAN HISTORICAL ASSOCIATION



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In This Issue

The articles in this issue take up questions of comparative history and explore the ways in which ideas originating in one part of the world affect and give conceptual shape to events (and perception of events) taking place elsewhere.

We begin with an *AHR Forum* on American exceptionalism and transnational history. The Australian scholar **Ian Tyrrell** argues that the notion of America as a special case retains power in U.S. historiography, despite the advent of comparative history, in part because of the stress in comparative analysis on national themes and traditions. It is high time, Tyrrell asserts, for the nation-centered focus to be supplemented by and situated in a transnational historical project. He proposes three emphases for that project: regions, world systems, and transnational processes associated with the global integration of culture and economy, such as economic, environmental, and social movements and international organizations.

Michael McGerr accuses Tyrrell of unnecessarily devaluing nation-centered and comparative history and of failing to make his case against notions of American distinctiveness. It is possible, McGerr contends, that American exceptionalism fosters the transnational ties Tyrrell wants to study. In any event, American historians of the exceptionalist persuasion are able to deal critically with the role of nationalism and nation-states in an interdependent world. Transnational history promises to have important, even radical implications for historical writing, McGerr concludes, but these should not come at the cost of rejecting national and comparative history. Ian Tyrrell offers a brief rebuttal.

Arthur Waldron tells us that the image of warlord and the concept of warlordism, both important categories in historical writing about modern China, originated not in Asia but in Europe. The image of warlord can be traced through cartoons and caricature from late nineteenth-century France through Germany, Japan, and the Soviet Union to the Chinese visual arts; the concept, which is tied to European theories of militarism, reached China from the West by way of Japan. When ideas of warlordism first came to China in the 1910s, they were felt to be in conflict with established notions about the place of violence in society. Only in the 1920s, after a series of destructive wars left Chinese intellectuals searching for new ways of defining the relationship of violence to society, did warlordism become an accepted category of political analysis.

John K. Thornton takes a fresh approach to identifying African influences in an American slave rebellion. Instead of the usual method of seeking "Africanisms" in America through examination of African culture, Thornton looks at specific events in Africa that touched the slaves who participated in the Stono Rebellion of 1739 in South Carolina. He finds that civil war in the Christian kingdom of Kongo produced the slaves who ended up in South Carolina at the time of the insurgency. They brought with them ritual practices and military tactics from Kongo that subsequently appeared in the events of the rebellion. White Americans who observed the rebellion naturally read these practices and tactics in a quite different way.

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AHR Forum
American Exceptionalism in an Age of
International History

IAN TYRRELL

IN AN ERA OF UNPRECEDENTED INTERNATIONALIZATION in historiography, the legacies of nationalism and exceptionalism still haunt the study of American history. History conceived as the origins and growth of the nation-state on the German model took root in many countries, yet nowhere has a nation-centered historical tradition been more resilient than in the United States. There, modern historicism, with its emphasis on the uniqueness of all national traditions, was grafted onto an existing tradition of exceptionalism. The pre-historicist idea of the United States as a special case “outside” the normal patterns and laws of history runs deep in American experience. Its origins, Dorothy Ross shows, lay in the merger of the republican and millennial traditions that formed an ideology of exceptionalism prominent in American historical writing. In this liberal world view, the United States avoided the class conflicts, revolutionary upheaval, and authoritarian governments of “Europe” and presented to the world an example of liberty for others to emulate.¹ This exceptionalist ideology persisted into the twentieth century, influenced such luminaries as Frederick Jackson Turner, and surfaced again in “consensus” historiography in the 1950s.²

The rise of historical specialization has shattered these confident assumptions

An earlier paper on this theme was delivered at the Convention of the Organization of American Historians, St. Louis, April 7, 1989. I wish to thank James Gilbert, Dorothy Ross, and John Higham for comments made at that session. I also thank Max Harcourt, Mark Berger, Michael Pearson, and other members of the School of History who commented on another version given to the School postgraduate seminar, April 22, 1989; and the anonymous critics for the *AHR*.

¹ Dorothy Ross, “Historical Consciousness in Nineteenth-Century America,” *AHR*, 89 (October 1984): 909–28. See also David Noble, *Historians against History* (Minneapolis, 1965).

² A useful recent summary of exceptionalism is in John Agnew, *The United States in the World-Economy: A Regional Geography* (New York, 1987), 8–15. The most prominent examples are found in the work of Turner and his followers. See, for example, Frederick Jackson Turner, *The Frontier in American History* (New York, 1920). Post-World War II historiography recapitulated important elements of the exceptionalist viewpoint. See Louis Hartz, *The Liberal Tradition in America* (New York, 1955); David Potter, *People of Plenty* (Chicago, 1954); Daniel Boorstin, *The Genius of American Politics* (Chicago, 1953). Hartz may be seen as a different case from some of the other key post-progressive historians. In *The Founding of New Societies* (New York, 1964), Hartz and his collaborators extended the interpretation of the liberal tradition as a national “fragment” of “European” political thought in a comparative dimension, illustrating national variations in the process. But it is not this book that exerts influence on American history. Rather it is the *Liberal Tradition*, which studies the United States not as a variation on a theme of the European fragment but as a single, unique case to be compared with “Europe.” This is derivative of Turner, Alexis de Tocqueville, and the exceptionalist tradition.

but put nothing convincing in their place. Even though many American historians today eschew exceptionalism, in the absence of an alternative organizing framework the vast bulk of U.S. history is still written in terms that accept the primacy of the national focus. More important, the exceptionalism of the liberal tradition has undergone a modest revival. In popular culture, exceptionalism remains strong, and in intellectual discourse, the debate over Paul Kennedy's *Rise and Fall of the Great Powers* (1987) shows how deeply ingrained are traditions of America as radically unique. In new work by Daniel Bell and Seymour Martin Lipset, the principles of exceptionalism are "reaffirmed." Postmodernist European observer Jean Braudrillard has followed in the tracks of earlier foreign critics to wonder at American uniqueness.³ In the fields of labor history, women's history, socialism, and foreign policy, important books have returned to the classic theme of Alexis de Tocqueville to emphasize once more American difference from Europe. Other historians avoid the language of exceptionalism assiduously but posit national difference as so central to American historiography that notions of exceptionalism are bound to be encouraged. All this despite the new social history, with its emphasis on the themes of gender, class, race, and ethnicity, themes that appear to cut across the central tenets of the faith in the United States of America as an exception.⁴

At the heart of this resilience of American exceptionalism is a paradox. To some critics, American historiography seems too narrow in its concentration on American uniqueness, yet in no other national historical tradition has comparative history received more rhetorical support.⁵ Alongside the belief in American exceptionalism, there has been in American historiography a marked impulse to relate American history to that of the rest of the world. To take one prominent example, much of Arthur Schlesinger, Sr.'s voluminous writing was concerned with what he called "national traits." In *Paths to the Present* (1949), he addressed the question raised in the eighteenth century by J. Hector St. John Crèvecoeur, "What

³ Paul Kennedy, *The Rise and Fall of the Great Powers: Economic Change and Military Conflict from 1500 to 2000* (New York, 1987); Norman Podoretz, "The Decline of America: A Best-Selling Theme," *Washington Post* (February 25, 1988): A25; Seymour Martin Lipset, "American Exceptionalism Reaffirmed," in Byron E. Shafer, ed., *Is America Different? A New Look at American Exceptionalism* (Oxford, 1991), 1–45; Jean Braudrillard, *America*, Chris Turner, trans. (London, 1988), 88–91; for Bell, see "'American Exceptionalism' Revisited: The Role of Civil Society," *The Public Interest*, 95 (Spring 1989): 38–56.

⁴ For work in labor history, see the replies to Sean Wilentz, "Against Exceptionalism: Class Consciousness and the American Labor Movement, 1790–1920," *International Labor and Working Class History*, 26 (Fall 1984): 1–24; Nick Salvatore, Michael Hanagan, and Steven Sapolsky, *ibid.*, 25–36; and 27 (Spring 1985): 35–38; Richard Oestreicher, "Urban Working-Class Political Behavior and Theories of American Electoral Politics, 1870–1940," *Journal of American History*, 74 (March 1988): 1257–86; for recent work in foreign policy history, see Akira Iriye, "Exceptionalism Revisited," *Reviews in American History*, 16 (June 1988): 291–97; for women, see Donald Meyer, *Sex and Power: The Rise of Women in America, Russia, Sweden, and Italy* (Middletown, Conn., 1987). For the emphasis on national difference rather than national uniqueness or exceptionalism, see Carl Degler, "In Pursuit of an American History," *AHR*, 92 (February 1987): 4.

⁵ Raymond Grew contended that American history has been more parochial than other historiographies; Grew, "The Comparative Weakness of American History," *Journal of Interdisciplinary History*, 16 (Summer 1985): 87–101. Grew conceded that his observation was partly based on experience as editor of *Comparative Studies in Society and History*—hardly a representative sample of American historical writing on comparative history. From the perspective of non-American historiography, American efforts seem substantial.

Then is the American, This New Man?" Alongside work unmistakably influenced by the exceptionalist tradition, however, Schlesinger wrote essays stressing America's global relationships. His warning is instructive: "History as conventionally written stresses national differences—even when not genuinely such—to the neglect of national similarities. This emphasis," Schlesinger noted, glossed "over the fundamental interdependence of peoples."⁶

Schlesinger's essay was first published in 1941 under the title "World Currents in American Civilization." Even then, as a glance at his bibliography shows, a substantial literature on American connections with "world history" existed.⁷ The presence of this strand of historical writing suggests that American exceptionalism has not gone unchallenged. A subordinate tradition of international analysis requires recognition in any account of American exceptionalism. This tradition offers instructive themes for the reconceptualization of national historiography in the United States and in other countries as well. American historians have been prominent in the drive to establish comparative history as a genre, for example, and have heralded such efforts as evidence of greater cosmopolitanism in American historical analysis. Technically, they are correct. The critical absence has not been comparative and international perspectives themselves but rather the failure of comparative history to transcend the boundaries of nationalist historiography. As an alternative, the possibilities of a transnational history must be considered. This does not mean that nationalism and the history of the nation-state can be ignored. It does mean that these national perspectives must be historicized and relativized by developing a new historiographical project organized in terms of a simultaneous consideration of differing geographical scales—the local, the national, and the transnational—in American historical thought.

The national focus I am criticizing here may seem merely to reflect a rock-bottom historical "reality."⁸ No one doubts the importance of both nationalism and the nation-state in the modern world. Yet, all too often, the primacy of these concepts is assumed by historians. This penchant for national frameworks reflects not merely the historian's common-sense observation of the contemporary world but also the way historical knowledge has been produced. History is not a set of data to be deposited into tidy boxes, of which the national box is the most obvious and sensible. History is, much more than most historians are willing to accept, a constructed body of knowledge.⁹ The role of the nation-state framework in the production of that history must be acknowledged, if alternative views of American history are to be advanced. Other disciplines have been much more open to transnational analysis than history has been, and even American historians, when they turn to the subject of European history, have made contributions

⁶ Arthur M. Schlesinger, Sr., *Paths to the Present* (New York, 1949), vii, 1, 169–85.

⁷ Schlesinger, *Paths to the Present*, 294–96; Arthur M. Schlesinger, "World Currents in American Civilization," in A. J. B. Wace, et al., *Studies in Civilization* (Philadelphia, 1941). See also Schlesinger, "History," in Wilson Gee, ed., *Research in the Social Sciences: Its Fundamental Methods and Objectives* (New York, 1929), 219–20.

⁸ Degler, "In Pursuit," 5.

⁹ Both the constructed nature of historical knowledge and the widespread unwillingness of historians to abandon their empiricism are clear from Peter Novick's *That Noble Dream: "Objectivity" and the American Historical Profession* (New York, 1988). See also Ian Tyrrell, *The Absent Marx: Class Analysis and Liberal History in Twentieth-Century America* (Westport, Conn., 1986).

to comparative and international history that demonstrate just how artificial the dominance of the national framework in American historiography can be.¹⁰ But these innovative achievements have not been fully appreciated by other national contingents of historians. This suggests that the problem lies less in the facts of American history or American perversity than in the nature of historical training. A focus on national difference is an occupational hazard among all historians, not just those of the United States. American exceptionalism presents a special case of the more general problem of history written from a national point of view.

NONE OF THIS WOULD BE WORTH DISCUSSING if American history were truly exceptional, but exceptionalism has always contained an insuperable logical difficulty. The history of the United States cannot be exceptional unless contrasted with other histories that conform to fixed patterns of historical development. In the twentieth century, American advocates of exceptionalism have, paradoxically, been hostile to the very idea of laws of historical development represented in, for example, Marxism, and have argued that these schematic accounts do not apply to American history. Yet such schemes of history underpin the explanations exceptionalist advocates give for American difference from other countries. The logical difficulty has been compounded by changes in the Marxist tradition itself. Marxists no longer hold to a rigid stages theory of historical development, and the fragmentation of the Marxist tradition has removed the contrast on which modern interpretations of exceptionalism have been constructed. Added to this, the exceptionalist tradition assumed an essentialist dichotomy between "America" and "Europe" that denies the complexity and variation European historians—Marxist and non-Marxist—have found in their own histories.¹¹

Many American historians have accepted these logical difficulties and argue instead either for national uniqueness or national difference. Since all national histories are unique, there is nothing objectionable about this maneuver, at least in principle. Yet "uniqueness" does have overtones of national superiority, and the concept has been used, for example by David Potter, in a sense that clearly implies exceptionalism. In his *People of Plenty* (1954), Potter reworked the familiar frontier thesis and explained differences between "Europe" and "America" in terms of American abundance. "Europe cannot think of altering the relationship between the various levels of society without assuming a class struggle; but America has altered and can alter these relationships without necessarily treating

¹⁰ See Charles Maier, *Recasting Bourgeois Europe: Stabilization in France, Germany and Italy in the Decade after World War I* (Princeton, N.J., 1975); Robert R. Palmer, *The Age of the Democratic Revolutions*, 2 vols. (Princeton, N.J., 1959–64); David Brion Davis, *The Problem of Slavery in Western Culture* (Ithaca, N.Y., 1966).

¹¹ There are many valuable critiques of exceptionalism: see Agnew, *United States in the World-Economy*; Aristide Zolberg, "How Many Exceptionalisms?" in Ira Katznelson and Zolberg, eds., *Working-Class Formation: Nineteenth-Century Patterns in Western Europe and the United States* (Princeton, N.J., 1986), 397–455; and most of the contributors to Jean Heffer and Jeanine Rovet, eds., *Why Is There No Socialism in the United States?* (Paris, 1988).

one class as the victim or even, in an ultimate sense, the antagonist of another."¹² This polarized and ahistorical treatment of two worlds defined by the presence or absence of class conflict echoed the preoccupations of earlier writers like Turner and the nineteenth-century originators of exceptionalism.

Even the historians who have emphasized national difference in more neutral fashion have helped perpetuate the concern with exceptionalism. To understand this, it helps to put individual historical works in the context of inherited historiography and keep in mind that far more history is written about the United States than any other country. If the United States is said to be different, the sum total of that research produces more evidence of specifically American difference. All histories may be distinctive, but American history becomes, through the sheer volume produced, "more distinctive" than others. Further, the focus on American difference cannot be divorced from the context of exceptionalism as an inherited ideology. The historian may deny the notion of exceptionalism, but his or her analysis reinforces the existing, deeply ingrained assumption that the history of the United States has been endowed with special features until the contrary is proven.¹³ For these reasons, exceptionalism is in practice inseparable from the concept of national distinctiveness in American historiography, and the two notions have become linked through comparative history. Although comparative history is by no means an American monopoly, the search to define, explore, and test the uniqueness of the American past has produced an impressive body of comparative history by world standards. For many years, this link was merely implicit. Advocates of exceptionalism such as Turner assumed American uniqueness rather than investigated it.¹⁴ More recently, the growth of comparative history, in part a product of the consensus historians' preoccupation with American uniqueness, has allowed systematic testing of exceptionalist ideas.¹⁵ Comparative history is for this reason not necessarily antagonistic to exceptionalism.

These connections are not always made clear by practitioners, and comparative history may sometimes be seen simply as a way of making the discipline in the United States more cosmopolitan.¹⁶ But even the supporters of comparative history acknowledge that the genre has its defects. The most obvious problem is the tendency to compare whole countries and to take for granted the primacy of the national unit of analysis. Thus slavery in the United States is compared to slavery in Brazil or to serfdom in Russia. Neither regional variations nor transnational influences are necessarily excluded in such accounts. Nevertheless,

¹² Potter, *People of Plenty*, 118. The *Concise Oxford Dictionary*, 1427, defines "unique" as "unmatched, unequalled, having no like or equal or parallel." This clearly has connotations of exceptionalism.

¹³ This is the problem with Degler's "In Pursuit," despite the sophistication and the nuances of his arguments about national comparison.

¹⁴ Turner, *Frontier*; Frederick Jackson Turner, *The Significance of Sections in American History* (1932; rpt. edn., Gloucester, Mass., 1950), 339.

¹⁵ John Higham, "Changing Paradigms: The Collapse of Consensus History," *Journal of American History*, 76 (September 1989): 465.

¹⁶ For surveys of comparative history, see George M. Fredrickson, "Comparative History," in Michael Kammen, ed., *The Past before Us: Contemporary Historical Writing in the United States* (Ithaca, N.Y., 1980), 457-73; George M. Fredrickson, "Giving a Comparative Dimension to American History: Problems and Opportunities," *Journal of Interdisciplinary History*, 16 (Summer 1985): 109; Peter Kolchin, "Comparing American History," *Reviews in American History*, 10 (December 1982): 79.

the historian is forced to subordinate variations in both time and space to the elaboration and explanation of larger national differences.¹⁷

Carried to an extreme, this type of comparative analysis reifies national characteristics. The classic case is the work of Louis Hartz, in *The Liberal Tradition in America* (1955) and *The Founding of New Societies* (1964). Here the liberal “fragment” derived from Europe’s more complex social structure determines the nature of political debate. The fragment becomes frozen and loses its dialectical relationship with other fragments to produce a self-perpetuating “tradition.” All major political and ideological developments can be explained in terms of such a national pathology.¹⁸

It is not necessary to deal with an entire national tradition in order to fall into this error. Some local case studies do likewise. A well-crafted and informative comparison by Norbert MacDonald of the divergent histories of Vancouver and Seattle illustrates the point. Here are two similarly situated cities. Geographical influences are relatively constant, therefore a comparison may reveal something significant about the national scene. Such a comparison suggested to this author striking differences “in local characteristics and processes” that reflected “the distinctive histories, roles and values of two separate nations.”¹⁹ Urban history, in this account, is national history writ small. At least one reviewer understood the message to be that such a comparison showed up the distinctive national features of American history identified with exceptionalism.²⁰ But how different would the perspective be if Vancouver were contrasted with other Canadian cities or port cities elsewhere in the world? We cannot know at present, because the research design in comparative history is narrowly conceived to test purely national differences rather than convey a more varied sense of the elements that make up the diversity of historical experience. The focus on national factors as an explanation of urban trends ignores the very real uncertainty among Canadians about their national culture and their frank recognition of regional differences. It also fails to take into account the degree of American cultural penetration that makes a sense of “Canadian” difference impossible to measure in terms of a two-way national comparative framework.

Most comparative history involves just two countries, but the choice of countries for comparison is as important as the number of countries studied. Take the recent example of Donald Meyer’s learned foray into feminist history, *Sex and Power* (1987).²¹ Meyer synthesized recent women’s history to ask, in effect, if the American women’s movement has been exceptional. Comparing the case of the United States with those of Sweden, the Soviet Union, and Italy, Meyer answered

¹⁷ Carl N. Degler, *Neither Black nor White: Slavery and Race Relations in Brazil and the United States* (New York, 1971); Peter Kolchin, *Unfree Labor: American Slavery and Russian Serfdom* (Cambridge, Mass., 1987).

¹⁸ Louis Hartz, *Liberal Tradition*; Hartz, *Founding of New Societies*.

¹⁹ Norbert MacDonald, *Distant Neighbors: A Comparative History of Seattle and Vancouver* (Lincoln, Neb., 1987), 195.

²⁰ Daniel E. Turbeville III, review of MacDonald, *Distant Neighbors*, in *Journal of American History*, 75 (March 1989): 1297. On Canadian-American difference, see the important work by Seymour Martin Lipset, *Continental Divide: The Values and Institutions of the United States and Canada* (New York, 1990).

²¹ Meyer, *Sex and Power*.

this question in the affirmative, but he did not make comparisons with Britain or its colonies, where similar movements for women's emancipation flourished. In this respect, Meyer's analysis can be contrasted with the older but still useful survey by Richard Evans in *The Feminists* (1977). A British historian of German history, Evans was not preoccupied with American national distinctiveness. He acknowledged important differences between the United States and some other key countries in the timing and scope of women's reform and feminist agitation but also stressed regional comparisons between, for example, the American West and some of the Australian colonies. More important, he grouped sets of countries together to produce a typology of movements for women's emancipation. Although his particular typology had the disadvantage of overemphasizing the "liberal" and "bourgeois" character of feminism in the Anglo-American world, his approach did enable him to set the question of American feminism in a broader international context that emphasized the American case as a variation on a theme.²²

Seymour Martin Lipset acknowledged the complication that the choice of countries introduces into comparative history. "Generalizations may invert when the unit of comparison changes. For example, Canada looks different when compared to the U.S. than when contrasted to Britain." Yet Lipset has not fully overcome the problem, as his own illustration shows. "Figuratively, on a scale of zero to one hundred, with the U.S. close to zero on a given trait and Britain at one hundred, Canada would fall around thirty. Thus, when Canada is evaluated by reference to the United States, it appears as more elitist, law-abiding, and statist, but when considering the variations between Canada and Britain, Canada looks more anti-statist, violent, and egalitarian."²³ But Lipset's own standard of comparison is revealed in his deployment of the numerical grid. Almost invariably in American scholarship, the benchmark is the United States. Comparisons between the other countries are either ignored or played down. This procedure cannot test notions of American exceptionalism adequately, and the outcome of the piling up of differences between the United States and a variety of other cases in a range of miscellaneous matters is to ensure that American exceptionalism is "reaffirmed."

This penchant for stressing national difference has some practical roots. Modern archives are organized nationally, and historians making comparisons have to rely to a large extent on these, as well as on the national historical traditions in each country that are in part a product of such mundane matters. Dependence on the existing monographic scholarship is particularly noticeable in comparative work done by historical sociologists and political scientists. By relying on national historiographies for the basic building blocks of comparative history, scholars do not validate exceptionalism but rather reflect the disciplinary traditions of the academic world they have inherited.

For Americanists, this means making the problematic of American exception-

²² Richard J. Evans, *The Feminists: Women's Emancipation Movements in Europe, America, and Australasia* (London, 1977), esp. 56–57, 62, 206, 214–15. See also Jane Rendall, *The Origins of Modern Feminism: Women in Britain, France and the United States, 1780–1860* (London, 1985), 320.

²³ Lipset, "American Exceptionalism Reaffirmed," 1–2. See also Seymour Martin Lipset, "Historical Traditions and National Characteristics: A Comparative Analysis of Canada and the United States," *Canadian Journal of Sociology*, 11 (1986): 113–55.

alist historiography central to comparative analysis. Much comparative history is written from the perspective of key issues in U.S. history. As Raymond Grew has perceptively noted, comparisons have "typically . . . dealt with issues that rise from within American historiography—frontiers, slavery, and immigrant groups . . . But rarely . . . has the exploration moved from the other direction, beginning with issues identified more fully in some other historical tradition to reveal something previously overlooked about American society . . . [E]ven the most successful systematic, transnational comparisons have led American historians back to the familiar mines rather than out toward new horizons."²⁴ Among those familiar mines, none is more prominent than the topic of American uniqueness.

It should be clear by now that the critics of exceptionalism cannot defeat the notion by exposing its illogicalities or by using the methodology of comparative history. The legacy of exceptionalism can only be laid to rest in two ways: by confronting the national focus of exceptionalist analysis, and by dealing with the special conditions of historical production that have shaped and sustained exceptionalism through the organization of historical knowledge in national units. This is why exceptionalism must be linked to national history and why the paradigm of national history must be rigorously scrutinized from the perspective of alternative transnational approaches. I do not mean to suggest that American history must be homogenized as part of some amorphous international history. The alternatives to national history I propose would contextualize nationalism and depict U.S. history as a variation on transnational themes.

THE NEW TRANSNATIONAL HISTORY could be constructed in several complementary ways. The first is a regional analysis drawing on the innovations and the inspiration of French *Annales* historiography and building on the earlier work done in various schools of local and regional historiography in the United States. In the celebrated work of Fernand Braudel, Emanuel LeRoy Ladurie, Marc Bloch, and others, national boundaries have been subordinated to the analysis of regional economies and viewpoints that reflect the local environment. These influences of climate, geography, outlook, and material life do not neatly follow the story of the rise of nation-states in the historiography of *Annales*.

American historians have praised *Annales* as a method of historical analysis, but they have not rushed to take up *Annaliste* themes. Where Braudel's example has been employed to elucidate the deeper structures of American social history, these structures are used in a typical example to explain assumed national

²⁴ Grew, "Comparative Weakness," 99–100. This inheritance is the product of more than such practical problems as the archival organization of research. Both theoretical issues and the intellectual inheritance are involved. The methodology of comparative history has had, since the nineteenth century, a strongly positivist tone. National units are assumed to be more or less self-contained specimens for historical analysis and comparison. Through such analysis, differences and similarities in national traditions can be ascertained in a method analogous to the laboratory testing of the natural sciences. The connections between the units considered for testing cannot be admitted or brought to the center of concern. For an excellent discussion of the varieties of comparative history, see A. A. Van Den Braembussche, "Historical Explanation and Comparative Method: Towards a Theory of the History of Society," *History and Theory*, 28 (1989): 1–24.

characteristics.²⁵ But even such limited applications are rare. Outside the colonial period, where studies of both demography and *mentalité* evidence an *Annaliste* influence, little at all has been done.²⁶ Why?

The scales of both time and place used by Braudel in *The Mediterranean* (1972–73) have not appealed to American historians because these do not square with the American historiographical preoccupation with the nation-state. The *longue durée* of Braudel's Mediterranean region has not seemed convincing to American scholars concerned with the themes of democratic politics and ideology thrown up by the American Revolution and inherent in the subsequent history of the making of the American nation. The dominance of Progressive historiography, symbolized in the work of Charles Beard and Mary Beard's *Rise of American Civilization* (1927) in the interwar period, suggests the vice-like grip these themes have had on professional historians and a wider public alike. Study of the material environment or enduring collective attitudes has not had appeal in a historiographical tradition concerned with rapid change and process rather than continuity and structure.²⁷

To be sure, precedents for a regional approach to the study of material life can be located in American scholarship. In the 1920s and 1930s, Walter Prescott Webb explored the history of the Great Plains as an environment shaping a regional tradition. Later, in *The Great Frontier* (1952), he extended his interest in the dynamic influence of frontier environments to a bold reinterpretation of Western civilization. Also in the interwar years, Herbert Eugene Bolton treated, on a slightly smaller scale, the transnational interaction of cultures in the Spanish borderlands and went on to propose a hemispheric approach to American studies. But Webb's attempt at a universal history of the frontier elicited ridicule from specialists in European and British history like J. H. Hexter, and even Webb's study of *The Great Plains* (1931) was denounced as overly deterministic.²⁸ Bolton, too, found that his efforts to make a regional approach the basis of a grand transnational historiography in "The Epic of Greater America" (1933) fell between American and Latin American history as conventionally conceived.²⁹ But there was a second reason inhibiting the reception of Bolton's thesis. Bolton's approach, which had its origins in work done before World War I, had relied

²⁵ Walter Nugent, *Structures of American Social History* (Bloomington, Ind., 1981).

²⁶ See, for example, James Henretta, "Families and Farms: *Mentalité* in Pre-Industrial America," *William and Mary Quarterly*, 3d ser., 35 (January 1978): 3–32.

²⁷ Richard Andrews, "Some Implications of the *Annales* School and Its Methods for a Revision of Historical Writing about the United States," *Review*, 1 (1978): 165–80; Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, Sian Reynolds, trans., 2 vols. (New York, 1972–73). Beard's work, though long superseded, still retains force as a powerful synthesis; Charles Beard and Mary Beard, *The Rise of American Civilization* (New York, 1927). The search for an interaction of material structures and political and ideological forms pervades a perceptive American critique of Braudel; see Bernard Bailyn, "Braudel's Geohistory—A Reconsideration," *Journal of Economic History*, 11 (Summer 1951): 277–82.

²⁸ Hexter wrote: "Professor Webb knows too little of the history of Europe to give him a sound basis for testing his bold hypothesis. It is probable that if he had known enough European history to test it, he would not have entertained the hypothesis in the first place"; *AHR*, 58 (October 1952): 963; Joe B. Frantz, "Walter Prescott Webb," in Wilbur R. Jacobs, John W. Caughey, and Joe B. Frantz, *Turner, Bolton, and Webb: Three Historians of the American Frontier* (Seattle, 1965), 90.

²⁹ Herbert E. Bolton, "The Epic of Greater America," *AHR*, 38 (April 1933): 448–74; John Francis Bannon, *Herbert Eugene Bolton: The Historian and the Man, 1870–1953* (Tucson, Ariz., 1978), 185–87.

primarily on looking at the sweep of American history from the colonial perspective. As modern American history loomed larger in the view of American historians, Bolton's work diminished in importance.³⁰ Bolton's thesis, like Webb's, failed because of the resistance of historians interested in the role national historical traditions played in the emergence of the modern world. In this sense, the pre-national history of what became the United States remained important to Americanists but chiefly as a condition for the emergence of national greatness.³¹

One regional project has produced a more formidable body of research than these: the Atlantic system. Beginning with the seminal work of Charles McLean Andrews, this "imperial" school of thought provided in the late nineteenth and early twentieth centuries a major point of reference for the study of the colonial era and the age of the American Revolution. Granted, Andrews's insistence on imperial connections gave way in the 1920s to the focus of the Progressive historians on the rise of the American nation with its internal class and sectional conflicts, but there are signs that the Atlantic is once again regarded in colonial studies as a very powerful framework.³² The Atlantic trading network is, for much of American history, a key region within which to explore links of a transnational kind. This need not entail the old project of the United States as an extension of European civilization or, still less, English colonialism. The Atlantic world must encompass Africa as well as Europe and the Americas and, as scholars like Gary Nash have argued, focus on the cultural and social interchange of Native Americans, Africans, African-American slaves, and a variety of European peoples. Such new studies must also transcend the Andrews school by focusing more on regional environmental factors than on political and institutional concerns. D. W. Meinig's magisterial *Atlantic America* (1986) did just this. He showed how the United States emerged from a set of disparate regions; each was carefully studied in its geographic peculiarities and relationships to the politics, culture, economy, and society of a larger Atlantic system of trade and the movement of peoples.³³

The study of slavery is one area that has benefited markedly from an Atlantic focus, beginning with Philip Curtin's pioneering work on the African slave trade. His work has influenced many American historians. Yet the barriers of nation-state historiography remain strong. The impressive series edited by Richard Price and titled "Johns Hopkins Studies in Atlantic History and Culture" has tended to neglect North American topics.³⁴ Perhaps this reflects the divisions of area

³⁰ John W. Caughey, "Herbert Eugene Bolton," in Jacobs, Caughey, and Frantz, *Turner, Bolton, and Webb*, 65–66.

³¹ See, for example, the way Kenneth Lockridge tied the work on colonial American demography into an interpretation of the origins of an American national character; Lockridge, "Social Change and the Meaning of the American Revolution," *Journal of Social History*, 6 (Summer 1973): 403–39.

³² Richard R. Johnson, "Charles McLean Andrews and the Invention of American Colonial History," *William and Mary Quarterly*, 3d ser., 43 (October 1986): 519–41. Andrews was, Richard Johnson correctly pointed out, "a persistent critic of those who, like Turner and his followers, seemed intent upon validating the study of American history by all but cutting it adrift from that of Europe"; p. 540.

³³ Gary Nash, *Red, White and Black: The Peoples of Early America* (Englewood Cliffs, N.J., 1974); D. W. Meinig, *The Shaping of America: A Geographical Perspective on 500 Years of History*, Vol. 1, *Atlantic America, 1492–1800* (New Haven, Conn., 1986).

³⁴ Philip D. Curtin, *The African Slave Trade: A Census* (Madison, Wis., 1969); Peter H. Wood, *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (New York, 1974);

specialization. Within U.S. history, slavery is still seen primarily as a unique phenomenon, to be compared with slavery in other national settings rather than as part of Atlantic history.

Despite the value of these studies of the Atlantic world, they are mostly concentrated in the seventeenth and eighteenth centuries, before the emergence of the American nation-state. Meinig has made clear that the successor volumes to his *Atlantic America*, dealing with the period after 1800, will return to a more traditional emphasis on what Americans have had in common rather than continue to focus on the regional diversity and Atlantic links that distinguished his first volume. This maneuver underscores the difficulty that even persistent advocates of geographical and environmental factors have in coming to terms with the cluster of ideological, social, and political developments transforming the American colonies into a nation in the age of the democratic revolutions. Any regional history with transnational implications faces this difficulty. Regional approaches are not going to be persuasive so long as they seem merely the precursors or the building blocks of a national story.

Another problem with the regional approach is the inability of any one regional frame of reference to encompass the sheer variety of transnational regional affiliations in American history. If Atlantic studies have been stressed, the hemispheric links identified by Bolton have been neglected. The impact of the United States on Latin America, the comparative history of revolution and state making in the New World, and the cultural interchange between Hispanic America and the United States in the "borderlands" region from Texas to California are key cases calling out for further historical investigation.³⁵ Another neglected regional framework is the continental focus pioneered in studies of Canadian-American relations done under the auspices of the Carnegie Endowment and directed by James T. Shotwell in the 1930s. Still another area of interest is the contacts of various kinds with East Asia and the Pacific.³⁶ While it would be valuable to see American historiography stress the variety of these regional frameworks, a geographic focus does not show how the American nation, particularly in the nineteenth century and after, has related to them and how the different regional transnational pressures on American history relate to one another. Both East Asian and Atlantic connections have been important, but, from the point of view of synthesis, the common denominator between Pacific and

Stanley L. Engerman and Eugene Genovese, eds., *Race and Slavery in the Western Hemisphere: Quantitative Studies* (Princeton, N.J., 1975). For excellent work in the Johns Hopkins series, see B. W. Higman, *Slave Populations of the British Caribbean, 1807-1834* (Baltimore, Md., 1984). An Atlantic system encompassing the North American colonies and the United States is more fully the subject of another volume in this series: Peggy Liss, *Atlantic Empires: The Network of Trade and Revolution, 1713-1826* (Baltimore, Md., 1983).

³⁵ See, for example, Gerald E. Poyo and Gilberto M. Hinojosa, "Spanish Texas and Borderlands Historiography in Transition: Implications for United States History," *Journal of American History*, 75 (September 1988): 393-416. For the colonial period, hemispheric studies overlap with the Atlantic focus. For stimulating work on this topic, see Nicholas Canny and Anthony Padgen, eds., *Colonial Identity in the Atlantic World, 1500-1800* (Princeton, N.J., 1987).

³⁶ Carl Berger, ed., *The Writing of Canadian History: Aspects of English-Canadian Historical Writing since 1900*, 2d edn. (Toronto, 1986), chap. 6; Akira Iriye, ed., *Mutual Images: Essays in American-Japanese Relations* (Cambridge, Mass., 1975). For another case, see James A. Field, Jr., *America and the Mediterranean World, 1776-1882* (Princeton, N.J., 1969).

Atlantic contacts has been the history of the United States. This is not a way to transcend the nation-state framework. Some transnational processes, moreover, have effects far beyond particular regions, even those as broad as the Atlantic system. There are strong arguments to be made in favor of approaches of a global kind, especially those that give proper place to local and national peculiarities.

The most influential attempt at this type of transnational historical analysis has come from Immanuel Wallerstein and the world-systems theory he has promoted. Building on the insights of both Marx and Braudel, Wallerstein discussed the rise of capitalism since the fifteenth century as a "world-economy" that is not coterminous with national borders. He recognized the dominance of particular nation-states at various times in the "core" of the system and was also able to explain, through his model of core versus periphery, variations in state structure and labor systems between the different parts of the "modern world-system." It is important to the survival of any world system that no one state remain dominant indefinitely, since that will tend in turn to produce a world empire in which the political and economic boundaries of the system are the same. This had happened with previous and transitory world economies but has yet not happened to the modern world system precisely because its basis is capitalist. That is, there is a functional geographical division in the system between economic and political activities. A capitalist world economy is superior to a world empire (and previous world economies) in terms of its ability to accumulate capital, because the tasks of policing the system are not done by any particular area. Core states can instead concentrate on the building of comparative economic advantages to advance capitalism and the world system as a whole. Wallerstein's approach has the advantage of combining both local conditions and systemic, transnational influences into one broad theory of historical change.³⁷

Despite its insights, Wallerstein's work has received only marginal endorsement from American historians.³⁸ Little effort has been made to explore the relevance of Wallerstein for specifically American topics, in which "America" is understood to mean what has become the United States. Yet world-systems theory is important because it enables us to put America's national and regional developments into better focus. The issue of American exceptionalism is largely meaningless within this framework. This is perhaps best seen in the recent work of the historical geographer John Agnew, in *The United States in the World-Economy* (1987).³⁹ Agnew charted the changing relationship of regions within the United States to the world economy and explained how and why the Northeast shifted from peripheral status in that international system in the eighteenth century to core status by the end of the nineteenth century, then was in turn challenged by other regions as the United States was progressively integrated into the world

³⁷ Three volumes have appeared so far; Immanuel Wallerstein, *The Modern World-System*, 3 vols. (New York, 1974–89). The most recent volume, *The Modern World-System: The Second Era of Great Expansion of the Capitalist World-Economy, 1730s–1840s* (New York, 1989), is of considerable relevance to American historians of the revolutionary era and synthesizes a large volume of American historical literature on this subject.

³⁸ But see Thomas McCormick, "World Systems," *Journal of American History*, 77 (June 1990): 125–32, which shows the relevance of the approach for foreign relations.

³⁹ Agnew, *United States in the World-Economy*, 8–15.

economy. For Agnew, the question of American power and its threatened decline in recent years is really a story of these shifting relationships to the world system. As Agnew himself emphasized strongly, the history of the United States seen in such a framework cannot be exceptional or unique. It is significant that such an insightful synthesis has come from a historical geographer.

Nonetheless, valid empirical and theoretical objections to Wallerstein's theory have been made.⁴⁰ Many scholars believe that Wallerstein's model is too rigid to cover the variety of social, cultural, and economic factors that explain how regional competitive advantages take shape within the system. Some critics from the Marxist tradition believe that only class forces within the emergent states can explain the process of capital accumulation and change in the capitalist system over time. As defined by Eugene Genovese and Elizabeth Fox-Genovese, the study of national political economies and their constituent class and ideological forces was undervalued by Wallerstein.⁴¹

If, because of the nature of his enterprise, Wallerstein was, on the one hand, insufficiently attentive to internal class and cultural factors, he did not, on the other hand, explore the range and complexity of international influences fully, either. Like other comparativists, he was largely dependent on the existing nation-state historiographies. Wallerstein synthesized this work in a most admirable way and related the economic history of states to the world economy, but he focused in that system on the impersonal economic force of the capitalist marketplace. Relatively little attention has been given by world-system theorists to concrete transnational institutions such as foreign trading companies and the specific financial and monetary mechanisms of the international market economy.⁴² Nor does world-systems theory tell us anything about noneconomic factors of a transnational kind that might influence the market.

⁴⁰ See Robert Brenner, "The Origins of Capitalist Development: A Critique of Neo-Smithian Marxism," *New Left Review*, 104 (1977): 25–92. Michael Pearson, *Before Colonialism: Theories on Asian-European Relations, 1500–1750* (Delhi, 1988); and Charles Ragin and Daniel Chirot, "The World System of Immanuel Wallerstein: Sociology and Politics as History," in Theda Skocpol, ed., *Vision and Method in Historical Sociology* (Cambridge, 1984), 302–05, 312, conveniently list and summarize major criticisms of Wallerstein. Empirical tests of world-systems theory in terms of the volume of international trade do not substantiate the importance of the world system, but the problem is precisely how to test such a complex entity in which political, economic, and social forces are interrelated. See Henry A. Gemery, review of *The Modern World-System III*, in *Journal of Interdisciplinary History*, 20 (Spring 1990): 638–41.

⁴¹ Eugene Genovese and Elizabeth Fox-Genovese, *Fruits of Merchant Capital: Slavery and Bourgeois Property in the Rise and Expansion of Capitalism* (New York, 1983), 71–72.

⁴² On this point, see the critique by Fred Block and Margaret R. Somers, "Beyond the Economistic Fallacy: The Holistic Social Science of Karl Polanyi," in Skocpol, *Vision and Method*, 73. On nineteenth-century international economic institutions, see the important but neglected work by Brinley Thomas in *Migration and Economic Growth: A Study of Great Britain and the Atlantic Economy*, 2d edn. (Cambridge, 1973), chap. 15; see also Mira Wilkins, *The Emergence of Multinational Enterprise: American Business abroad from the Colonial Era to 1914* (Cambridge, Mass., 1970); Mira Wilkins, *The Maturing of the Multinational Enterprise: American Business abroad from 1914 to 1974* (Cambridge, Mass., 1974); for the eighteenth century, see Jacob Price, *France and the Chesapeake: A History of the French Tobacco Monopoly, 1674–1791, and of Its Relationship to the British and American Tobacco Trades*, 2 vols. (Ann Arbor, Mich., 1973).

BECAUSE OF THE DIFFICULTIES IN USING WORLD-SYSTEMS THEORY, historians interested in transnational connections should consider two strategies: one is to revive the various regional projects discussed above, the other and perhaps more revolutionary move would be to strike out in the direction of a global focus more attentive to historical specificity and variety than Wallerstein's largely economic model. It is this second approach that I now want to consider in some detail. European expansionism has gradually created a global economy and is today in the process of creating a global society as well. These developments in the spread of European values, technologies, peoples, and power began in the sixteenth century and encompass the opening of the Americas, although the evolution of a specifically global pattern of relations is typically tied by scholars to the era of late nineteenth-century imperialism. As Stephen Kern showed, this change to a global outlook included culture as well as technology and heralded new ways of thinking about both time and space.⁴³

Global pressures are evident in a number of ways: the emergence of a set of interdependent economic relations to which American history could be connected; the related development of communications systems and technology; global environmental constraints, both natural and constructed; systems of alliances and political blocs; the international movements and organizations, both governmental and nongovernmental, that have influenced nation-states; and the growth since the turn of the century of obligations under international law that, in some cases, override national laws. These processes are creating a global context that demonstrates the inadequacy of a national framework for comprehending the present historical circumstances of the United States and that could form the basis of a new approach to important aspects of the American past. I do not propose to discuss each of these in detail.⁴⁴ Instead, I shall focus first on economic connections, then the environment, and finally organizations, ideologies, and movements to illustrate how American history can be related to transnational influences and how such influences point up the weakness of exceptionalist explanations.

For the development of a transnational project using an American base, the study of the interaction of the American Colonies and republic with the international economic system remains crucial, whatever the weaknesses found in

⁴³ Stephen Kern, *The Culture of Time and Space, 1880–1918* (Cambridge, Mass., 1983), esp. chap. 8; Eric Hobsbawm, in *Industry and Empire: An Economic History of Britain since 1750* (London, 1968), 112, discussed the economic terms of this new nineteenth-century globalism. Hobsbawm described the British as creating a "kind of planetary system circling around the economic sun of Britain." See also Hobsbawm, "The Missing History—A Symposium," in *Times Literary Supplement* (June 23–29, 1989): 690. The most cogent assertion of the importance of a transnational history similar to the one I am advocating here comes in a few remarks by Perry Anderson, in "Agendas for Radical History," *Radical History Review*, 36 (1986): 35–37.

⁴⁴ I am indebted to David Held, "The Decline of the Nation State," in Stuart Hall and Martin Jacques, eds., *New Times: The Changing Face of Politics in the 1990s* (London, 1989), 35–37, for this formulation. The study of these processes is by no means the preserve of historians alone. In the field of international politics, see, for example, Robert O. Keohane and Joseph S. Nye, Jr., *Transnational Relations and World Politics* (Cambridge, Mass., 1973); David Calleo, *Beyond American Hegemony: The Future of the Western Alliance* (New York, 1987); and Held, "Beyond the Nation State." American historians could, by looking at American international connections from the perspective of social, economic, and cultural history, complement the work being done on international history from the perspective of world politics.

Wallerstein's model. Because of the growing divorce between economic history and general history since the 1960s, the insights of the older traditions of economic history that stressed international connections have not been consolidated. What exactly has been the role over time of such critical economic factors as trade, investment, and immigration? Even though the United States built a strong national economy in the nineteenth century, international influences must not be underestimated. Much dispute exists over the importance of international trade vis-à-vis the development of a national market. Among economic historians, critics of transnational approaches argue that the role of international trade in the core economies of the world system in the eighteenth and nineteenth centuries was far from spectacular.⁴⁵ The tendency to stress domestic factors is most tempting of all for econometric historians, who are preoccupied with measuring national incomes and rates of national economic growth.⁴⁶

These choices involve theoretical assumptions on the part of American economic historians that indicate they, too, are not free from the legacy of American exceptionalism. Most recent syntheses emphasize the vast internal market and natural resources of the nineteenth-century United States and treat the European economies in a positivistic way as a purely external stimulus to be added to the internal factors of land, capital, and labor as variables. To be sure, there have been dissenters. Brinley Thomas's study of Atlantic migration vigorously contested this paradigm and focused instead on the interrelation of economic growth, migration, and capital formation in an international economy. That his thesis has often been criticized or, alternatively, ignored by modern American economic historians suggests the extent to which they are prepared to work within a national framework of economic analysis.⁴⁷

The importance of trading interdependence cannot be easily discounted. In the colonial era, the volume of export trade ranged from 20 to 30 percent of gross national product (GNP), according to the best estimates, and the presence of an imperial system based on mercantilism made the political economy of "foreign trade" of greater importance than even this high volume of trade would suggest. For later periods, the significance of foreign trade declined from its high levels of the colonial and early republican era, and exports as a percentage of GNP never exceeded 7 percent in the nineteenth century. But this figure is rarely set in comparative perspective. In Britain, the ratio of foreign trade to GNP was scarcely

⁴⁵ See, for example, Richard P. Thomas and Donald McCloskey, "Overseas Trade and Empire, 1700–1860," in Roderick Floud and Donald McCloskey, eds., *The Economic History of Britain since 1700*, vol. 1 (Cambridge, 1981), 102; Patrick O'Brien, "European Economic Development: The Contribution of the Periphery," *Economic History Review*, 2d ser., 35 (February 1982): 1–18, esp. 4.

⁴⁶ Lance E. Davis, et al., *American Economic Growth: An Economist's History of the United States* (New York, 1972), is typical in its stress on endogenous factors; see esp. 554–61; Susan Previant Lee and Peter Passell, in *A New Economic View of American History* (New York, 1979), dealt with the international economy in a most cursory manner, focusing only on trade (146–52). This emphasis in economic history on states can be traced to the work of an influential pioneer of the theory of economic "growth," Simon Kuznets. See his "The State as a Unit in the Study of Economic Growth," *Journal of Economic History*, 11 (Winter 1951): 25–41.

⁴⁷ Thomas, *Migration and Economic Growth*. It is true that Jonathan Hughes, *American Economic History*, 2d edn. (Glenview, Ill., 1987), 298, argued that "Thomas' grand thesis . . . has withstood well the test of critical examination by scholars for nearly three decades and remains intact." Hughes, however, gave more extended attention to international trade, immigration, international financial institutions, and foreign capital than most American economic historians do.

higher, and Patrick O'Brien's study noted that in 1790 only 4 percent of Europe's GNP was exported across national boundaries. Nonetheless, most observers concede that international trade grew at an impressive rate within the Atlantic economy and provided a notable stimulus to the system in the nineteenth century.⁴⁸ Simon Kuznets long ago argued persuasively that foreign trade has followed cyclical patterns, with much of the nineteenth century characterized by unprecedented "high rates of growth."⁴⁹ *Historical Statistics of the United States* (1976) commented correctly that the "United States was more heavily dependent upon foreign markets and sources at that time than it has been in the 20th century."⁵⁰ Moreover, it is the composition of that trade that is most important. Key sectors like agriculture in the nineteenth century and manufacturing since World War I have been relatively more involved in international trade than other sectors. Cotton before the Civil War is a well-known case, since 50 percent of American exports by value at that time were provided by the cotton economy. The political and economic ramifications of cotton's position in the American economy for national politics (and international diplomacy) are widely recognized, but the purely economic aspects of the cotton trade require further research. Douglass North's analysis of the crucial role of cotton exports has been subjected to severe criticism from some econometric historians, but little work has been done on the overall significance of the cotton export trade and its interrelation with finance, shipping, foreign capital imports, and immigration.⁵¹ For the late nineteenth century, the impact of farm politics and the search for markets for agricultural products has been extensively treated, thus testifying to the continuing importance of foreign agriculture in American political economy.⁵² In the twentieth century, the shift to an exchange of sophisticated industrial commodities has

⁴⁸ For the United States, trade was particularly important with Britain up to at least 1860, in ways that ratios to GNP do not reveal. See Jack Potter, "Atlantic Economy, 1815–1860: The U.S.A. and the Industrial Revolution in Britain," in A. W. Coats and R. M. Robertson, eds., *Essays in American Economic History* (London, 1969), 14–48. The figures on American exports are taken from David, *American Economic Growth*, 554. For comparative statistics and further sources on international trade, see Thomas and McCloskey, "Overseas Trade and Empire," 102; O'Brien, "European Economic Development," 1–18.

⁴⁹ Quoted in Edward L. Morse, "Transnational Economic Processes," in Keohane and Nye, *Transnational Relations*, 38. Morse has further valuable arguments for the historical importance of transnational economic processes. See also Simon Kuznets, *Modern Economic Growth: Rate, Structure, and Spread* (New Haven, Conn., 1966).

⁵⁰ I have used this publication in the retitled 1976 edition, with introduction by Ben J. Wattenberg, *The Statistical History of the United States from Colonial Times to the Present* (New York, 1976), 876.

⁵¹ Douglass North, *Economic Growth of the United States, 1790–1860* (New York, 1966), 75–77; D. P. Crook, *The North, the South, and the Powers, 1861–1865* (New York, 1974). Susan Previant Lee and Peter Passell, *New Economic View*, attacked Douglass North's contention that external demand for cotton was the driving force in southern economic expansion, yet they focused only on North's interregional trade argument that southern cotton specialization stimulated midwestern farmers to supply grain to cotton producers. They failed to assess other aspects of international economic interdependence, such as the role of immigrant labor. They also conceded, ironically, that "the dependence of the East on western foodstuffs does offer some indirect confirmation of North's thesis. Southern cotton production generated export revenues used by the East to purchase foreign capital, and thereby to specialize in nonagricultural production"; p. 151. For a summary of arguments against the North thesis, see also Stanley Engerman and Robert E. Gallman, "U.S. Economic Growth, 1790–1860," *Research in Economic History*, 8 (1983): 1–46. Engerman and Gallman accepted the paradigm of the nation-state as the focus of economic growth, as first articulated by Kuznets; pp. 2–3.

⁵² William Appleman Williams, *The Roots of the Modern American Empire* (New York, 1969).

perpetuated the interdependence of the world and American economies, though in different ways than in earlier periods. The reliance of the United States on Third World countries for key raw materials required for a complex industrial economy has increased markedly since the 1920s and forms an underpinning to the hegemonic political role assumed by the United States since World War II.⁵³

It is possible to go further to suggest the importance of long waves of economic change in the cycles of world trade, investment, and economic growth. This theme can be tied to the integration of the United States into the capitalist world economy. A comparison of American wholesale price indexes with the long wave cycles of international capitalist development originally proposed by the Russian economist N. D. Kondratieff shows a remarkable concurrence of cyclical trends in the American and world economies since the late eighteenth century.⁵⁴ Although the internal market was large, the United States developed this market in the nineteenth century with the help of heavy importation of capital and labor, factors that made the international connections of the American economy vital. The importance of British capital flows in the nineteenth-century American economy was long ago suggested by the work of Alec Cairncross, and the impact of international financial fluctuations has been charted, for the turbulent 1830s, by Peter Temin.⁵⁵ Nor should migration be neglected. The existence of abundant land or resources meant nothing without the labor to develop those resources, as well as the technologies and know-how that immigrants brought with them. Even if immigration studies have moved away from consideration of migration as a flow of human capital, the work of Brinley Thomas on the economic significance of international migration remains largely sound.⁵⁶ The United States was, like some West European countries in the 1970s, able to "export" part of its unemployment problem by massive repatriation of Mediterranean labor in the era of classic imperialism before World War I. The benefits in reducing the social costs of industrialization are difficult to calculate, but they must be taken into account when assessing American abundance and economic growth as somehow exceptional in the nineteenth century.⁵⁷ The United States built its strong national

⁵³ This is a point cogently made by Gabriel Kolko, *Main Currents in Modern American History* (New York, 1976), 197–98, 383–86.

⁵⁴ In the case of the late 1830s, Peter Temin, *The Jacksonian Economy* (New York, 1969), stressed international financial connections as the root cause of fluctuations in the American economy. For price and cyclical correlations, see Agnew, *United States in the World Economy*, 22. In Carville Earle, "The Myth of the Southern Soil Miner: Macrohistory, Agricultural Innovation, and Environmental Change," in Donald Worster, ed., *The Ends of the Earth: Perspectives on Modern Environmental History* (Cambridge, 1988), 182 and following, the cycle is applied to the prerevolutionary Tidewater as well.

⁵⁵ A. K. Cairncross, *Home and Foreign Investment, 1870–1913* (Cambridge, 1953); Temin, *Jacksonian Economy*; Mira Wilkins, *The History of Foreign Investment in the United States to 1914* (Cambridge, Mass., 1989).

⁵⁶ Hughes, *American Economic History*, 298; Darwin H. Stapleton, *The Transfer of Early Industrial Technology to America* (Philadelphia, 1987), stresses the importation of human capital as a boost to technological know-how.

⁵⁷ The importance of repatriation as a factor in American social and economic development has not been given the attention it deserves. See Betty Boyd Caroli, *Italian Repatriation from the United States, 1900–1914* (New York, 1973); Theodore Saloutos, *They Remember America: The Story of the Repatriated Greek-Americans* (Berkeley, Calif., 1956); Alexander Keyssar, *Out of Work: The First Century of Unemployment in Massachusetts* (Cambridge, 1986), 78–79. Keyssar stated the position nicely: "Workmen in Europe and Canada constituted a reservoir of labor that was tapped when needed and that reabsorbed jobless workers when business was slow in Massachusetts"; p. 79.

economy using international labor supplies and capital of vast extent, and it relied—more than in the period 1913–1950—on international trade and a buoyant world economy; thus the very era in which American ideas of exceptionalism were most prominent is revealed as an era of economic interdependence.

This global economic focus is valuable in part because it provides essential background for assessing the arguments made for American isolation, free security, and abundance that litter the discourse of American historiography.⁵⁸ At the same time, a focus on the relationship between international economic development and shifts in the American economy raises important issues for social, political, and cultural development. Most obviously, the ebb and flow of immigration facilitated the interchange of ideas and cultural values. Far too often, the question of immigration has been seen in terms of assimilation and upward mobility, as one would expect in exceptionalist historiography. The cultural meanings of large-scale repatriation of immigrants, which often reached a third of the total number of immigrants, have been largely ignored.⁵⁹ Equally important is the impact of the international economy on regional subcultures and on classes and social structures. For the colonial period, one authority has commented, “transatlantic commerce altered the structure of communities by raising living standards and widening the gap between those at the top and the rest of colonial society.”⁶⁰ The impact of external factors on domestic development may be both less and different for later periods, yet regional disparities of wealth and power remain a marked feature of the United States that might be linked to the fluctuations of the world economy.⁶¹

A SECOND MAJOR AREA THAT ILLUSTRATES THE POTENTIAL for a transnational approach is environmental history. Because modern environmental movements have had strong American roots, environmental history is, in the United States, a promising and sophisticated historical specialty. Yet Donald Worster has noted sadly that some environmental historians have, in order to gain acceptance in a profession dominated by the idea of history as the study of nations and national cultures, “already gone far to mold” themselves “to fit the national boundaries mindset.” His call is for an alternative synthesis that “moves easily across national

⁵⁸ C. Vann Woodward, “The Age of Reinterpretation,” *AHR*, 66 (October 1960): 2–8.

⁵⁹ See, for example, how repatriation is discussed purely in terms of an American impact on the countries to which immigrants returned in Carl Degler, *Out of Our Past: The Forces That Shaped Modern America*, 3d edn. (New York, 1984), 301–02. A fascinating account of how migration patterns can reveal much about networks of international communication is found in the work of two German historians, Dirk Hoerder and Harmut Keil, “The American Case and German Social Democracy at the Turn of the 20th Century, 1878–1907,” in Heffer and Rovet, *Why Is There No Socialism in the United States*, 141–63.

⁶⁰ J. M. Sosin, review of Ian K. Steele, *The English Atlantic, 1675–1740: An Exploration of Communication and Community* (New York, 1986), in *William and Mary Quarterly*, 3d ser., 44 (October 1987): 800. A study in this vein is Paul G. Clements, *The Atlantic Economy and Colonial Maryland's Eastern Shore: From Tobacco to Grain* (Ithaca, N.Y., 1980). For a related approach emphasizing the differential importance of export-led sectors in the colonial period, see John J. McCusker and Russell Menard, *The Economy of British America, 1607–1789* (Chapel Hill, N.C., 1985).

⁶¹ Agnew, *United States in the World-Economy*.

boundaries.”⁶² Environmental damage has not been confined within narrow and often arbitrary political boundaries; drought, industrial pollution, land settlement, and the overuse of natural resources such as marine life, soil, and water could be studied in a wider, transnational framework. Doing so could build on work done by the Canadian economic historian Harold Innis and others in the 1930s.⁶³ Because environmental damage has not been confined within national boundaries, environmental ideas and movements have often been transnational in scope.⁶⁴ Most important, investigations of environmental ideas and contexts could comment on American exceptionalism by inquiring into the transnational ecological consequences of American “abundance.” To what extent has American wealth and power been acquired by transferring to other peoples the environmental costs of economic development?⁶⁵

Studies of systemic connections in trade, labor, immigration, investment, and environmental constraints leave open the possibility that shared material processes were interpreted differently in the United States. Transnational similarities of structure may confound American exceptionalism, only to give way to profound differences of meaning at the ideological and cultural levels. Since much of American historiography is taken up with the exploration of cultural meanings and social and political activism, exceptionalism could still be rescued at the level of consciousness. In part, this pitfall might be avoided by historicizing American exceptionalism as a set of ideas in cultural history. Dorothy Ross has done this, for American social science theory, by tracing the intellectual crisis of exceptionalism in the Gilded Age, when immigration and class conflict made

⁶² Donald Worster, “World without Borders: The Internationalization of Environmental History,” in Kendall E. Bailes, ed., *Environmental History: Critical Issues in Comparative Perspective* (Lanham, Md., 1985), 662–63. Worster has advanced this project in a fine collection that illustrates the possibilities for comparative and transnational analyses of environmental topics: Worster, *Ends of the Earth*; see also Worster, “Transformations of the Earth: Toward an Agroecological Perspective in History,” *Journal of American History*, 76 (March 1990): 1087–1106.

⁶³ The focus in this case would be a North American one for much of what has conventionally been “American” history. See the studies in the Carnegie Series on Canadian-American relations described in Berger, *Writing of Canadian History*, chap. 6; Harold A. Innis, *The Cod Fisheries* (New Haven, 1940).

⁶⁴ As long ago as the mid-nineteenth century, environmentalist ideas had a transnational focus. George Perkins Marsh, the Vermonter who wrote the seminal work *Man and Nature* (1864), gleaned major insights from comparing the experience of land use in the New World with that in the Old, especially during his years as an American diplomat in Italy in the 1860s. *Man and Nature*'s search for the causes of ecological degradation was of great interest to scientists in many countries, and topics like the effects of the destruction of the forests on climate and soil erosion on rivers and seas had obvious global implications. Given the wealth of his data and the breadth of his insights, Marsh's reputation was an international one. The impact of his ideas abroad has not yet been studied by historians. The way is open in the case of Marsh's work for a comparative study of conservation that pays attention to the flow of ideas across national boundaries as well. David Lowenthal, *George Perkins Marsh: Versatile Vermonter* (New York, 1958), chap. 13. This point is confirmed in Michel F. Girard, “Conservation and the Gospel of Efficiency: Un Modèle de gestion de l'environnement venu d'Europe?” *Histoire sociale/Social History*, 23 (May 1990): 63–80.

⁶⁵ The most obvious example is the treatment of American Indians, which is rarely seen as a transnational phenomenon. But see Francis Jennings, *The Invasion of America: Indians, Colonialism, and the Cant of Conquest* (Chapel Hill, N.C., 1975). On the ecological impact of European settlement, see Alfred W. Crosby, *The Columbian Exchange: Biological and Cultural Consequences of 1492* (Westport, Conn., 1972); Alfred W. Crosby, *Ecological Imperialism: The Biological Expansion of Europe, 900–1900* (New York, 1986). A recent, widely acclaimed study of the interaction of environmental change and Native American–European relations is William Cronon, *Changes in the Land: Indians, Colonists, and the Ecology of New England* (New York, 1983).

many elite Americans fear that their society was no longer unique.⁶⁶ If exceptionalist ideas have their own history in this way, they cannot be assumed to have been hegemonic in all periods. The power of exceptionalism to explain American circumstances has varied over time and must be assessed against alternative ideologies. Among these ideological influences, the impact of transnational sentiments must be addressed.

THE THIRD ASPECT OF A TRANSNATIONAL research project requiring urgent attention is the study of organizations, movements, and ideologies. Since the time of de Tocqueville, the American republic has been characterized as a nation of joiners. Despite this emphasis on American uniqueness in the study of voluntary organizations, a large number of international associations with American connections await historical treatment. Little American research has been done on such organizations, even though an exhaustive survey of the internationalist impulse in Europe has demonstrated that they were ubiquitous in the nineteenth century.⁶⁷ Many of these organizations have had American leadership or support; for instance, reform movements such as the Good Templars and the Woman's Christian Temperance Union, working-class organizations such as the Industrial Workers of the World, social service organizations such as Rotary International, church and missionary groups such as the United Society for Christian Endeavor, and scientific and academic groups such as the International Geodesic Association. These groups need to be investigated to see to what extent their internationalism was genuine and to what extent they represented a spread of American influence and cultural hegemony. Either way, they illustrate the profound international connections of modern American society.⁶⁸

As the case of international organizations suggests, the entire ideology of "internationalism" as a concept requires examination. Problematic as the concept may be, the study of internationalism is important as a context for other types of transnational action and for developing a perspective on the limitations of the nation-state framework. Many manifestations of internationalism have had strong roots in American experience, yet American historians have largely ignored this evidence. Such a focus would not neglect nationalism. Indeed, the very concept of nationalism as a motivating ideology implies both localism and internationalism as its points of contrast. The study of internationalist ideologies would be particularly important in relation to political elites, intellectuals, and reform movements. The international dimension has been unavoidably confronted in histories of the peace crusade but often neglected in the case of other reforms. It is true that a

⁶⁶ Dorothy Ross, *The Origins of American Social Science* (New York, 1991). See also Nell Irvin Painter, *Standing at Armageddon: The United States, 1877–1919* (New York, 1987); Ian Tyrrell, "Women and American Exceptionalism," paper delivered at the biennial conference of the Australian and New Zealand American Studies Association, July 1990.

⁶⁷ F. S. L. Lyons, *Internationalism in Europe, 1815–1914* (Leyden, 1963).

⁶⁸ A start can be made by looking at William T. Stead's *The Americanization of the World; or, The Trend of the Twentieth Century* (London, 1902); Emily Rosenberg, *Spreading the American Dream: American Economic and Cultural Expansion, 1890–1945* (New York, 1982); James A. Field, Jr., "Transnationalism and the New Tribe," in Keohane and Nye, *Transnational Relations*, 3–22.

start has been made, often by British historians, on the topic of transatlantic reform. Frank Thistlethwaite's pioneering work has been consolidated in such areas as abolitionism and revivalism.⁶⁹ But in the late nineteenth century, a host of reform groups reflected the imperial context and aspired toward a global, not just an Atlantic, focus. They constituted themselves as specifically international organizations, campaigned on transnational themes, fostered international contacts, and promoted their own versions of international ideologies. The International Woman Suffrage Alliance (IWSA) started by Carrie Chapman Catt was a prominent example among women's groups.⁷⁰ Sometimes, as in the case of the World's Woman's Christian Temperance Union, these women's organizations have mobilized grass-roots support and identified women in other lands as part of an "international sisterhood."⁷¹

Such expressions of internationalism have usually been associated with elites. Yet transnational ideologies have at times had wider purchase. Working-class participation could be explored in such rituals of the socialist and labor movements as the May Day celebrations of the European working classes in the late nineteenth century. The American Federation of Labor staged such a May Day demonstration in 1890, and similar celebrations proliferated among immigrant workers who made up much of the American working class in the Progressive Era.⁷² Internationalist sentiment among workers could also be traced back into earlier periods of American history. The creation of strong nationalist sentiments as an aspect of popular culture has been largely a product of the nineteenth century; prior to that time, it was laboring people like the seaman of Marcus Rediker's fine study, *Between the Devil and the Deep Blue Sea* (1987), who carried with them in their lives and work the baggage of a transnational cultural system. National identities that cut across such allegiances grew during the era of the American Revolution and national consolidation thereafter, but national traditions had to be patiently constructed, indeed, "invented," in the language of Eric

⁶⁹ Frank Thistlethwaite, *The Anglo-American Connection in the Early Nineteenth Century* (Philadelphia, 1959); Richard Carwardine, *Transatlantic Revivalism, Popular Evangelicalism in Britain and America, 1790–1865* (Westport, Conn., 1978); Betty Fladeland, *Men and Brothers: Anglo-American Antislavery Cooperation* (Urbana, Ill., 1972).

⁷⁰ On international organizations, see especially Lyons, *Internationalism in Europe*, 263–307, 309–61. This material is primarily European, although there are some American references. For an American example, see Ian Tyrrell, *Woman's World/Woman's Empire: The Woman's Christian Temperance Union in International Perspective* (Chapel Hill, N.C., 1991). A comprehensive and critical study of Catt's international activities and the IWSA is needed. In the meantime, see Mary Gray Peck, *Carrie Chapman Catt: A Biography* (New York, 1944); Edith Hurwitz, "The International Sisterhood," in Renate Bridenthal and Claudia Koonz, eds., *Becoming Visible: Women in European History* (Boston, 1977), 327–45.

⁷¹ Tyrrell, *Woman's World/Woman's Empire*; Hurwitz, "International Sisterhood," 327–45; Evans, *Feminists*, 246–53.

⁷² Lyons, *Internationalism*, 182, states that the American Federation of Labor planned a demonstration for 1890; Joseph Rayback, *A History of American Labor*, rev. edn. (New York, 1966), 195, argues that the demonstration was part of a campaign for the eight-hour day and largely misfired. The question of internationalism was addressed in the papers in the session, "May Day Observances among Immigrant Workers, 1890–1930: Socialism, Ethnicity and Internationalism, Conflicting or Complementary Themes," at the Organization of American Historians' Convention, Washington, D.C., March 1990. See also, for illuminating comments, Eric Hobsbawm, "Mass Producing Traditions: Europe, 1870–1914," in Eric Hobsbawm and Terence Ranger, eds., *The Invention of Tradition* (Cambridge, 1983), 283–85.

Hobsbawm and Terence Ranger.⁷³ Even in the era after the emergence of American nationalism, deprived groups such as African Americans continued to harbor dreams of transnational ideologies. The long history of Back-to-Africa movements testifies to this point.

These examples suggest that the United States—the home of an influential ideology of exceptionalism—may, paradoxically, be a prime site for the study of transnational ideologies. Something similar was suggested long ago by Randolph Bourne in an important but neglected essay, titled “Trans-National America” (1916). Bourne rejected the melting pot ideology in favor of a more multicultural approach to the problems of American nationalism and immigration. He saw the United States as the scene of a cosmopolitan interchange that involved a simultaneous consideration of national and transnational allegiances, “a weaving back and forth, with the other lands, of many threads of all sizes and colors.”⁷⁴ Bourne’s essay has been treated as the outpouring of a literary eccentric, but his view bears a striking resemblance to the thought of Jane Addams in her *Newer Ideals of Peace* (1907). Addams’s time in the Chicago settlement house movement did not lead her to emphasize national xenophobia or exclusiveness. Rather, immigrants were in the slums “laying the simple and inevitable foundations of an international order” through the cosmopolitan “intermingling of the nations.”⁷⁵

The process of immigration may have made social reformers like Jane Addams more inclined toward internationalism and peace agitation than they would otherwise have been. The international connections of American development on the economic, social, and cultural planes may be responsible for the proliferation of “international” movements and sentiments in the United States. But the irony was the close association between such versions of American internationalism and the ideology of exceptionalism. Bourne, in particular, could not conceive of any other nation as the site of such a cosmopolitan interchange. The United States was “a unique sociological fabric.” This approach suggests that any consideration of international ideologies starting from an American base must still come to grips with the specific roles of the nation-state and nationalism in the articulation of those very dreams for transnational futures. In turn, the emphasis in American forms of internationalism on the outreach of specific cultural institutions rooted in American experience indicates how closely “internationalism” has been linked to American exceptionalism through concepts of cultural expansionism. Bourne saw the United States as helping through its diverse society the uplift of “uncivilized” peoples. In the process of repatriation of immigrant groups, he saw not the activities of “the parasitic alien” but the mechanisms of Americanization abroad. “They return [to their homelands] with an entirely new critical outlook, and a sense of the superiority of American organization to the primitive living around them.” In such judgments, Bourne’s cosmopolitan vision becomes con-

⁷³ Marcus Rediker, *Between the Devil and the Deep Blue Sea: Merchant Seamen, Pirates, and the Anglo-American Maritime World, 1700–1750* (Cambridge, 1987); Wilbur Zelinsky, *Nation into State: The Shifting Symbolic Foundations of American Nationalism* (Chapel Hill, N.C., 1988); Hobsbawm and Ranger, *Invention of Tradition*.

⁷⁴ Randolph Bourne, “Trans-National America,” in Bourne, *The History of a Literary Radical and Other Papers by Randolph Bourne*, Van Wyck Brooks, ed. (New York, 1956), 283.

⁷⁵ Jane Addams, *Newer Ideals of Peace* (New York, 1907), 18.

fused and scarcely rises above the level of cultural imperialism advocated by some missionary groups and their supporters.⁷⁶ If a history of transnationalism is to have any meaning, therefore, it must explore the complex dialectic between exceptionalism and internationalism. Only this dialectic explains the enthusiasm among intellectuals—and academic historians, no less—for the study of an American character on the one hand and an insistence on the interdependence of peoples on the other. Arthur Schlesinger's remarks in *Paths to the Present* were in this respect neither atypical nor contradictory but indicative of the complexities of the relations between the American nation-state and its international environment.⁷⁷

HOWEVER TRANSNATIONALISM is approached in American history, the question of its relationship to the history of the nation cannot be avoided. This is a problem for all historiographies, because of the importance of the nation-state in the modern world, but, for much of American history, it is a crucial point. The United States may be enmeshed in a global interdependence, but the relationship of U.S. history to transnational themes is different from that of many—though not all—other countries. A great power does not have the same relation to systems of international law, organizations, or movements that a small power does. Transnational analysis must therefore confront the problem of hegemony on the international level. This is not the same as saying that the United States is exceptional, since power is transitory and has shifted markedly over the course of the five hundred years of European exploration and settlement of the Americas. Nationalism and the state must still be historicized, and in so doing exceptionalism may be put in its proper place.

How can the nation-state be incorporated in this project? American historians interested in developing a transnational approach must specify the relations of three phenomena: first, the international context of national action in all of its manifestations. This would include not only international economic relations but also transnational connections in religion, culture, and social life; second, the development of the nation-state constrained by these international contexts; third, the groups and classes that operate both within the nation-state and at the international level. A key example would be the way certain American women reformers at the turn of the century, dispirited in some measure by the failure to achieve the vote in the United States, established international feminist connections and proposed that feminism be the basis of a new internationalism. The failure of the American nation-state to give equal citizenship rights made some feminists hostile to nationalism. Said Ellen Sargent, a California suffragist, in 1910: "who that realizes the situation can be patriotic? . . . Our own *United States* have the *home made material* of which anarchists are made." In her study of

⁷⁶ Bourne, "Trans-National America," 272, 282–83.

⁷⁷ Schlesinger, *Paths to the Present*. In a recent study of movements for world government, Wesley Wooley noted the "paradoxical nationalism" of those he termed "supranationalists." "[O]ne-worlders have seldom neglected American national interests or been hesitant to suggest that reformed global institutions be patterned after American experience"; Wooley, *Alternatives to Anarchy: American Supranationalism since World War II* (Bloomington, Ind., 1988), ix.

Feminism in Germany and Scandinavia (1915), Katharine Anthony expanded on the point: "The disenfranchisement of a whole sex, a condition which has existed throughout the civilized world until a comparatively recent date, has bred in half the population an unconscious internationalism. The man without a country was a tragic exception; the woman without a country was the accepted rule." The result? "The enfranchisement of women now under way" had in Anthony's view "come too late to inculcate in them the narrow views of citizenship which were once supposed to accompany the gift of the vote. Its effect" would, she predicted, "rather be to make the unconscious internationalism of the past the conscious internationalism of the future."⁷⁸

Anthony's predictions went astray, but that does not make her sentiments unimportant. The goals of feminists were diverse; their aims to bolster international solidarity between women and to provide compensation for losses at home were salient ones. Yet international ideology and organization also served to achieve goals at the national level. American women stepped in this case outside the boundaries of the state in order to put pressure on it by manipulating international opinion.

The choice was not necessarily a clear one between international and national action. Both planes of activity could be pursued simultaneously; what on one level is an internationalist strategy could also have force in building a social movement in the United States. Take the case of Pan-Africanism as practiced by Marcus Garvey's Universal Negro Improvement Association (UNIA). This ideology could be interpreted as much as an attempt to create a racial solidarity that could aid upward social mobility as a concrete endeavor to leave the United States. The Garvey movement was rooted in a long tradition of African-American emigrationist sentiment. Migration was not necessarily out of the country; it is arguable that such migration had its psychological and ideological dimension and that its spatial dimensions depended on particular circumstances. Thus southern black "exodusters" of the 1870s went to Kansas; a later generation of ghetto migrants continued to reject racism and socioeconomic deprivation in different ways through the "Back-to-Africa" movement. Local social conditions proved fertile for the development of Pan-African ideology as a response to an intellectual climate of European colonialism and racism.⁷⁹ In this way, the local history of social movements can be linked to international contexts.

This three-tiered model of social action can illuminate the relationship between transnational patterns on the one hand and the development of the state and modern nationalism on the other. Such an approach would tie in with the concern of political scientists and sociologists such as Theda Skocpol to examine the relationship between class forces and the growth of state power in comparative

⁷⁸ Ellen Sargent to Caroline Severance, January 1, 1910, Box 23, Caroline Severance Papers, Huntington Library, San Marino, Calif.; Katharine Anthony, *Feminism in Germany and Scandinavia* (New York, 1915), 3–4.

⁷⁹ Judith Stein, *The World of Marcus Garvey: Race and Class in Modern Society* (Baton Rouge, La., 1986), stresses the international connections of Garveyism. For the use of black Zionism to build racial solidarity in the United States, see E. David Cronon, *Black Moses: The Story of Marcus Garvey and the Universal Negro Improvement Association* (Madison, Wis., 1955), 185–87. On the millennial tradition in black migration within the United States, see, for example, Nell Irvin Painter, *Exodusters: Black Migration to Kansas after Reconstruction* (New York, 1977).

perspective. What needs to be added to their reconceptualization of the role of the state is a stronger sense of the interplay between domestic and international forces in the shaping of state power. Transnational factors do not simply define the possibilities for social action within the state in the way that Skocpol has indicated; classes and groups have on occasion moved beyond state boundaries to contribute to the international forces shaping and limiting state power.⁸⁰ This perspective helps contextualize American exceptionalism. The particular constellation of international and domestic class and group forces gives the state its specific character. In this way, American national power can be historicized by relating it to the changing balance of these forces. The growth of the American nation-state can thereby be depicted not as an exception to patterns of national power in a world of nations but as a particular, and constantly changing, expression of complex forces.

All this is a formidable task for any group of historians yet not an impossible one. The chief obstacles are practical and are connected to the resilience of historiography written from the national point of view. The detailed strategy of execution would demand a separate essay, since changes in historians' ways of producing knowledge are required, but the main points can be swiftly indicated. The growing strength of international connections in the contemporary world will surely make historians reconsider these relationships in past times. The internationalization of scholarship itself is steadily eroding the boundaries that at the turn of the century created strong national historiographical traditions, including American exceptionalism. In part, historians could achieve much along the lines suggested were they simply to build on work already done by the Americanists on international history, comparative history, and regional history discussed in this essay. A new focus on the interplay of global connections and local variations would bring much of this older work into a more helpful relation to current political and economic concerns in ways that would by-pass the temptation to revive exceptionalism. But other strategies are required as well. More could be done by scholarly associations and universities to create institutional frameworks in which these issues could be addressed. Certainly, such tasks cannot be accomplished through individual action alone. Pooling the talents of historians to explore transnational themes is needed.⁸¹ More than at any time since the turn of the century, American historians are alive to the potential for a historiography that transcends national boundaries. Yet only with institutional support can the momentous changes occurring today in global relations be matched in American historical writing by a new "age of international history."

⁸⁰ Theda Skocpol, Dietrich Rueschemeyer, and Peter B. Evans, eds., *Bringing the State Back In* (Cambridge, 1985); a stimulus to my argument, suggesting the importance of international connections, is Block and Somers, "Beyond the Economic Fallacy," 73–75.

⁸¹ I have in mind the cooperation evidenced in Canny and Padgen, *Colonial Identity*, in which common social processes are treated in a range of local variations and the emergence of national identity is not assumed but treated as contingent. Some practical suggestions have been canvassed in an essay that is congenial to my argument: Akira Iriye, "The Internationalization of History," *AHR*, 94 (February 1989): 1–10, esp. 2–3.

The Price of the “New Transnational History”

MICHAEL MCGERR

IAN TYRRELL IS FOR SO MANY OF THE RIGHT THINGS—global consciousness, “new” history, environmental history, more “institutional support” for historians. And he is against the right things, too—reification, “exceptionalism,” American-centered scholarship, national-based historiography. These are all sound positions for historians of the United States in the 1990s, living in an interconnected but ecologically threatened world and writing about a declining but still prideful nation. Most of Tyrrell’s central arguments have been made before, but surely no one has sounded the call for an internationalized historical writing, a “new transnational history,” with more daring or more commitment.¹

Tyrrell is correct: we ought to write about the transnational as well as the local and the national; we ought to assess the international influences on the history of the United States. But he asks us to pay a heavy—and perhaps needless—price for this “new transnational history.” We are called on to surrender or at least de-emphasize (it is not always clear which), not only notions of American “exceptionalism” but also the possibility of American distinctiveness, the pursuit of comparative history, and the practice of nation-centered historical writing. These are costs we might be willing to accept if Tyrrell could adequately justify them. But he does not. He creates a false antagonism between allegedly exceptionalist American historians and more progressive transnational writers. He does not deal with the evidence for American national exceptionalism or distinctiveness. In fact, he offers some support *for* exceptionalism. Ironically, American distinctiveness may have contributed to the making of the transnational world he describes. And the purported exceptionalists he attacks may offer a useful example for the writers of the “new transnational history.” In the end, Tyrrell’s determination to fight exceptionalism, American distinctiveness, comparative studies, and nation-centered history prevents him from exploring more fully the potential of an internationalized historiography.

ALTHOUGH TYRRELL SEEMS TO ARGUE THAT different forms of historical writing can coexist, he nevertheless builds his case for transnational history on a critique

¹ Laurence Veysey, “The Autonomy of American History Reconsidered,” *American Quarterly*, 31 (Fall 1979): 455–77; Akira Iriye, “The Internationalization of History,” *AHR*, 94 (February 1989): 1–10.

of the idea of American exceptionalism. He does not define exceptionalism directly, but the term appears to entail three propositions: an insistence on American divergence from "fixed patterns of historical development" and "laws of historical development"; a belief that this divergence is unchanging and perpetual; and some "confident assumptions" about American "national superiority."²

This is a useful definition, but it leads to problems. How many contemporary historians actually argue for this kind of American exceptionalism? Some social scientists—notably Daniel Bell and Seymour Martin Lipset—proclaim themselves exceptionalists. Yet it is difficult these days to find any historian who will do the same. American historians typically invoke "exceptionalism" when they want to attack, not defend, the idea of the nation's uniqueness. To borrow a title from Sean Wilentz, we stand "Against Exceptionalism." The idea that the United States is significantly different from, let alone superior to, other nations is something we would rather leave out back on the cold war scrap heap. Any historian caught near that pile of outmoded notions has to be ready with a disclaimer or an apology. In his presidential address to the American Historical Association in 1986, Carl Degler renounced "the specter of American exceptionalism" in order to propose comparative studies that would isolate the particular character of American life. "The purpose, I emphasize, is not to praise us," he reassured his audience, "but to understand who we are." Dorothy Ross, having documented the tenacious hold exceptionalist ideology had on the pioneering American social scientists of the nineteenth and twentieth centuries, was in a worse fix. Caught with the goods, she had to "plead guilty to a species of American exceptionalism." But she made "clear . . . lest there be any room for doubt, that my examination of the ideology of American exceptionalism is a critique of this idea of American uniqueness, not an endorsement . . . [M]y intention in singling out this ideology for historical critique is to render it less effective in the future."³

Unable to find much recent exceptionalism, Tyrrell has to look back in time to historical writing from the cold war: *The Genius of American Politics* by Daniel Boorstin; *The Liberal Tradition in America* by Louis Hartz; *People of Plenty* by David Potter; and "The Age of Reinterpretation" by C. Vann Woodward. This seems like a sensible strategy. Boorstin, Hartz, and Potter are the usual suspects, rounded up whenever the profession wants to repudiate "consensus history" one more time. Woodward, of course, rejected consensus, but he, like the others, wrote about the distinctive nature of the United States. Accordingly, all four

² Ian Tyrrell, "American Exceptionalism in an Age of International History," *AHR*, 96 (October 1991): 1034, 1031, 1034.

³ Daniel Bell, "'American Exceptionalism' Revisited: The Role of Civil Society," *Public Interest*, 95 (Spring 1989): 38–56; Seymour Martin Lipset, "A Unique People in an Exceptional Country," *Society*, 28 (November/December 1990): 4–13; Sean Wilentz, "Against Exceptionalism: Class Consciousness and the American Labor Movement, 1790–1920," *International Labor and Working Class History*, 26 (Fall 1984): 1–24; Carl N. Degler, "In Pursuit of an American History," *AHR*, 92 (February 1987): 3, 7; Dorothy Ross, *The Origins of American Social Science* (Cambridge, 1991), xviii. For an unusual contemporary embrace of exceptionalism by an American historian, see Harold M. Hyman, *American Singularity: The 1787 Northwest Ordinance, the 1862 Homestead and Morrill Acts, and the 1944 G. I. Bill* (Athens, Ga., 1986). Significantly, Hyman used the term "singularity" to try to distance himself from exceptionalism.

historians appear to be safe targets for Tyrrell. He faults Hartz for reification. Potter's *People of Plenty*, merely a reworking of "the familiar frontier thesis," is "polarized and ahistorical." The works of Potter and Woodward, Tyrrell declares contemptuously, "litter the discourse of American historiography."⁴

Unfortunately, when we pick up and read some of this litter, it becomes clear that not all four of these historians fit Tyrrell's definition of exceptionalism. It is not only that they customarily eschewed the term exceptionalism and avoided mention of any laws of history. Boorstin certainly believed in America's distinctiveness, but the others were more cautious. Potter contended "that the United States enjoyed a richer endowment than other countries and that this physical heritage has influenced our past history and our present society in distinctive ways." But he was no spread-eagle exceptionalist. Speaking at Oxford in the late 1940s, Potter noted approvingly that "the tendency of American historians of an earlier period to emphasize those phases of the national record which seemed unique" was giving way to "the gradual development, more recently, of a less restricted outlook stressing the factors which mark America as part of the Western world." Woodward also placed limits on the distinctiveness of the United States. "It should go without saying," he wrote, "that American civilization is European derived." Even Hartz circumscribed the uniqueness of the United States. "Ultimately . . . for all of the magical chemistry of American liberal society," he observed, "we are dealing with social materials common to the Western world."⁵

Nor did all four consider American distinctiveness perpetual and unchanging. Potter may have. *People of Plenty* gives no sign that its author believed America's unique abundance would come to an end. Hartz and Boorstin both argued that the special character of the country had been determined almost from the beginning of white settlement. But Hartz wrote in the hope that the nation's attachment to liberalism might be replaced by "a new level of consciousness, a transcending of irrational Lockianism." Mindful of the "larger forces" at work in the cold war world, Hartz was unsure of the future: "There is nothing in an analysis of American history which gives us a final answer to this question; we are left hanging in the air concerning it." The cold war also led Boorstin to wonder whether the United States, for all its "givenness," might change. He wrote *The Genius of American Politics* partly in the fear that Americans could fail "to conserve the institutions and the genius which have made America great." Woodward had no doubts at all about the impermanence of American distinctiveness. He wrote "The Age of Reinterpretation" to argue that America's long era of "free security," provided by its relative geographic isolation, had come suddenly and decisively to

⁴ Tyrrell, "American Exceptionalism," 1034, 1035, 1048. Daniel J. Boorstin, *The Genius of American Politics* (Chicago, 1953); Louis Hartz, *The Liberal Tradition in America: An Interpretation of American Thought since the Revolution* (New York, 1955); David M. Potter, *People of Plenty: Economic Abundance and the American Character* (Chicago, 1954); C. Vann Woodward, "The Age of Reinterpretation," *AHR*, 66 (October 1960): 1-19.

⁵ Potter, *People of Plenty*, 139; *The South and the Sectional Conflict* (Baton Rouge, La., 1968), 153; Woodward, "Age of Reinterpretation," 15; Hartz, *Liberal Tradition in America*, 17. One might also note that Woodward praised Walter Prescott Webb's *Great Frontier*, the book Tyrrell considers a harbinger of transnational history sadly excoriated by historians. See "Age of Reinterpretation," 15.

an end with the development of atomic bombs and intercontinental ballistic missiles.⁶

Whatever their belief in American distinctiveness, these four historians were not entirely tribunes of "national superiority." Boorstin was undoubtedly the most chauvinistic of the group—although we should not miss the irony of an intellectual lauding his country for having "nothing in the line of a theory that can be exported to other peoples of the world." But Hartz, Woodward, and Potter certainly pondered America's special nature in part because they were troubled by their nation and by nationalism. Writing in light of "the entire crisis of our time," Hartz condemned "the American liberal language," with its "vast and almost charming innocence of mind," and deplored the "two-edged effect of the American liberal absolutism—hampered insight abroad and heightened anxiety at home." Hartz doubted that distinctive American liberalism offered much to the rest of the world. "Hence the question is not whether our history has given us something to 'export' but whether it has given us the right thing," he concluded. "And this question has to be answered in the negative." More acerbic, Woodward turned "The Age of Reinterpretation" into a catalog of American faults: "the sunnier side of the national disposition—the sanguine temperament, the faith in the future"; "the disposition to put living standard, private indulgence and wasteful luxury ahead of vital security requirements"; "the irresponsibility of political leaders"; "demagogic diplomacy and military shortcomings"; "peculiar national attitudes toward power"; "the faltering and bewildered way in which America faced its new perils and its new responsibilities." Above all, Woodward, echoing Reinhold Niebuhr, punctured the "national myth that America is an innocent nation in a wicked world."⁷

Potter also looked critically at nationalism and its implications. "[E]xaggerated nationalism," he wrote, "is one of the greatest evils of the modern world." Potter recognized that national feeling could disfigure historiography, as he made plain in his essay "The Historian's Use of Nationalism and Vice Versa": "the valuative elements in the concept of nationalism have influenced too many of the findings of the historian, . . . [T]he concept has warped his analysis as much as it has assisted it." Believing that "national history . . . retains a vital importance," Potter still urged the historian to "be constantly mindful of the forces which link his nation to the world community." This nuanced treatment of nationalism and national history hardly evokes the "confident assumptions" of exceptionalism.⁸

Rereading Potter and the others is not only a matter of fairness and accuracy. The work of these four historians does not turn out to loom like some nationalist obstacle that must be swept aside for us to reach the promised land of transna-

⁶ Hartz, *Liberal Tradition in America*, 308; Boorstin, *Genius of American Politics*, 189; Woodward, "Age of Reinterpretation," 2–8.

⁷ Boorstin, *Genius of American Politics*, 1; Hartz, *Liberal Tradition in America*, 5, 7, 288, 305; Woodward, "Age of Reinterpretation," 5–8. See also John Patrick Diggins, "Knowledge and Sorrow: Louis Hartz's Quarrel with American History," *Political Theory*, 16 (August 1988): 355–76. Boorstin did write more critically about the United States in *The Image: Or What Happened to the American Dream* (New York, 1962). Stephen J. Whitfield considers the distinctive place of this book in Boorstin's writings in "The Image: The Lost World of Daniel Boorstin," *Reviews in American History*, 19 (June 1991): 302–12.

⁸ Potter, *South and the Sectional Conflict*, 35, 83, 152.

tional history. The exceptionalist label may apply to Boorstin, but it manifestly does not describe Hartz, Potter, and Woodward. One begins to suspect that Tyrrell is creating an issue where none really exists.

This suspicion is reinforced when we look at the authors he cites in support of transnational history. Understandably, the *Annales* school provides useful lessons for American historians, always vulnerable to European sophistication. "In the celebrated work of Fernand Braudel, Emanuel LeRoy Ladurie, Marc Bloch and others," Tyrrell declares, "national boundaries have been subordinated to the analysis of regional economies and viewpoints that reflect the local environment."⁹ But Braudel, at least, did not always treat "national boundaries" in this way. Tyrrell's Braudel is the historian who wrote *The Mediterranean and the Mediterranean World in the Age of Philip II* and *Civilization and Capitalism*; he is not the historian who wrote *The Identity of France*. In that final, uncompleted work, Braudel came, he himself said, "rather late in the day to my home ground, though with a pleasure I will not deny . . . I have saved my white bread until last." Braudel had no doubts about the legitimacy of his focus on the French nation. "Europe and the world are indeed participants in our past, crowding in on us and sometimes crushing us," he wrote. "But it should be understood that for no nation does the obligatory and increasingly burdensome dialogue with the outside world mean an expropriation or obliteration of its own history. There may be some intermingling but there is no fusion." Sure of the national past, Braudel was almost mystically certain of the national future. Since World War II, he confessed, "I have never ceased to think of a France buried deep inside itself, within its own heart, a France flowing along the contours of its own age-long history, destined to continue, come what may." This confident nationalism, this celebration of national history, is no reproach to American exceptionalists, whoever they may be.¹⁰

Tyrrell also summons world-systems theory in the fight against exceptionalism. "The issue of American exceptionalism is largely meaningless within this framework," he insists. "This is perhaps best seen in the recent work of the historical geographer John Agnew . . . As Agnew himself emphasized strongly, the history of the United States seen in such a framework cannot be exceptional or unique."¹¹ But Agnew's book, *The United States in the World-Economy: A Regional Geography*, is hardly an unqualified rejection of exceptionalism. Like Braudel, Agnew is comfortable with both transnationalism and national distinctiveness. "American history, therefore, is different," Agnew contends, "but it is also critically dependent on America's interactions with the world-economy." Even as he insists on international analysis, Agnew makes a considerable concession to exceptionalism. "The American case for exceptionalism," he concludes, "is often most persuasive." When transnationalists uphold national history and American distinctive-

⁹ Tyrrell, "American Exceptionalism," 1038.

¹⁰ Fernand Braudel, *The Identity of France: Volume 1, History and Environment*, Siân Reynolds, trans. (New York, 1988), 15, 20–21, 24–25; *The Mediterranean and the Mediterranean World in the Age of Philip II*, 2 vols., Siân Reynolds, trans. (New York, 1972–73); *Civilization and Capitalism, 15th–18th Century*, 3 vols., Siân Reynolds, trans. (New York, 1981–84).

¹¹ Tyrrell, "American Exceptionalism," 1042, 1043.

ness, when American historians avoid exceptionalism, we do not have the makings of irreconcilable intellectual conflict.¹²

EXCEPTIONALISM, HOWEVER RARE ITS ADVOCATES, plays a crucial role in Tyrrell's arguments. Not only is exceptionalism fatally flawed, its "legacy" distorts such seemingly unobjectionable historical projects as comparative studies of the United States and other nations. The influence of exceptionalism is evidently so perverse and so pervasive that we must purge national history and comparative history, along with exceptionalism.

Tyrrell rests his case against exceptionalism not on evidence but on logic. He declares that "exceptionalism has always contained an insuperable logical difficulty. The history of the United States cannot be exceptional unless contrasted with other histories that conform to fixed patterns of historical development." Moreover, American exceptionalists "have, paradoxically, been hostile to the very idea of laws of development."¹³ Exceptionalists, that is, have been inconsistent as well as illogical. We might ask whether historians who do believe in "laws of historical development" could make an acceptable argument for exceptionalism. The answer would seem to be "no," because Tyrrell apparently rejects the notion of "laws" and "fixed patterns" in history. But why do we need to traffic in "laws" and "fixed patterns" in the first place? Why cannot historians make a case that over some span of time some aspect of American experience has differed from the experience of other countries? Historians would be then making claims only about what has actually happened in the past and not about what must happen, according to some "laws," in the future. In short, why cannot historians consider whether the United States has simply been "different," not "exceptional"? This project is what Degler clearly had in mind in his presidential address to the American Historical Association.

But "difference" is also unacceptable to Tyrrell. Here again, he does not make a case on the basis of evidence, on data revealing the similarity of the United States to other nations. Instead, he argues that studies of difference only reinforce our false exceptionalism. Even American historians who "avoid the language of exceptionalism assiduously," Tyrrell claims, nonetheless "posit national differences as so central to American historiography that notions of exceptionalism are bound to be encouraged." He suggests two reasons for this. First, as he points out, "far more history is written about the United States than any other country. If the United States is said to be different, the sum total of that research produces more evidence of specifically American difference. All histories may be distinctive, but American history becomes, through the sheer volume produced, 'more distinctive' than others."¹⁴ American historians seemingly should stop considering the question of national difference because their work might create the wrong

¹² John Agnew, *The United States in the World-Economy: A Regional Geography* (New York, 1987), 8, 15.

¹³ Tyrrell, "American Exceptionalism," 1034.

¹⁴ Tyrrell, "American Exceptionalism," 1035.

impression. We apparently cannot trust people to read carefully, to weigh evidence and argument; they will simply be overawed by tall stacks of books.

The second argument against "difference" has a similar anti-intellectual quality. Tyrrell maintains that "the focus on American difference cannot be divorced from the context of exceptionalism as an inherited ideology. The historian may deny the notion of exceptionalism, but his or her analysis reinforces the existing, deeply ingrained assumption that the history of the United States has been endowed with special features until the contrary is proven."¹⁵ Once again, we must censor ourselves because our readers cannot be trusted to get the right message. This argument for the enduring influence of exceptionalism is particularly ironic given Tyrrell's criticism of Louis Hartz for insisting on the enduring influence of liberalism. Accusing Hartz of reification, Tyrrell rejects the contention that liberalism, long ago inherited from Europe, could become "frozen" and "produce a self-perpetuating 'tradition.'" Further, he denies that "all major political and ideological developments can be explained in terms of such a national pathology."¹⁶ But apparently we can have "such a national pathology" in American historiography. Once exceptionalist, U.S. historical writing is always exceptionalist. And so we must abandon the study of national difference. Exceptionalism is like some toxic waste that has poisoned the ground of American history: we can only avoid contamination by evacuating the land, rowing our lifeboats out into safe, transnational waters.

Perhaps a more rigorous comparative history, scrupulous in its assessment of national differences, could shield us from the toxic effects of exceptionalism and allow us to continue with our work as American historians. But comparative history turns out to be inadequate as well. It tends, Tyrrell notes, "to take for granted the primacy of the national unit of analysis . . . [T]he historian is forced to subordinate variations in both time and space to the elaboration and explanation of larger national differences."¹⁷ By its nature, then, comparative history supports the nationalist perspectives that give rise to exceptionalism. Presumably, the comparative method must also go.

Lost in this dismissal of exceptionalism, difference, comparative history, and national history is any consideration of evidence for or against American exceptionalism and distinctiveness. Over the years, historians have cited such factors as the frontier, the "presence" of John Locke and the absence of feudalism, abundance, the federal system, and the luxury of free security to explain alleged differences between the United States and other nations. Tyrrell does not dispute any of these factors. Moreover, his article suggests yet another source of American difference—the ideology of exceptionalism itself. "In popular culture," he observes, "exceptionalism remains strong."¹⁸ John Agnew also contended that "the ideology of American exceptionalism is an integral part of American history." This ideology has had consequences. Dorothy Ross has shown how exceptionalist values crucially affected the evolution of American social

¹⁵ Tyrrell, "American Exceptionalism," 1035.

¹⁶ Tyrrell, "American Exceptionalism," 1036.

¹⁷ Tyrrell, "American Exceptionalism," 1035–36.

¹⁸ Tyrrell, "American Exceptionalism," 1032.

science.¹⁹ In our own time, we recognize the impact of various exceptionalist ideas. The myth of America as a uniquely middle-class society tends to mute people's awareness of class differences. The "American dream," the less than fully justified faith in social mobility, tends to reconcile both the more and less fortunate to the inequalities of capitalism. The "national myth that America is an innocent nation in a wicked world," noted by Woodward in 1960, continues to shape the foreign policy of the United States, as President George Bush's pursuit of war with Iraq might suggest. Tyrrell himself recognizes the danger here. "Transnational similarities of structure may confound American exceptionalism, only to give way to profound differences of meaning at the ideological and cultural levels," he observes. "Since much of American historiography is taken up with the exploration of cultural meanings and social and political activism, exceptionalism could still be rescued at the level of consciousness."²⁰ And naturally, if we believe that consciousness has roots in society, in "structure," then we are back with the question of whether or not the United States differs from other countries.

That may not be such a bad thing, even for "the new transnational history." One of the most interesting aspects of Tyrrell's article is the suggestion of a connection between exceptionalism and internationalism. Here, Tyrrell makes use of Randolph Bourne's essay "Trans-National America," published in 1916. In a world torn by European war, in a nation rent by crusades for "Americanization," Bourne dared to look toward a tolerant future in which Americans accepted one another in their diversity and nations lived peaceably together. The striking thing about the essay is Bourne's argument that an exceptionalist United States would lead the way to this internationalist future. A more unabashed exceptionalist than Tyrrell indicates, Bourne believed in the singularity of the United States and the glory of its destiny. America, composed of so many immigrants from around the world, was "a novel international nation, the first the world has seen . . . [I]t bespeaks a poverty of imagination not to be thrilled at the incalculable potentialities of so novel a union of men." In particular, the United States would set an example of peaceable heterogeneity. "Only America," Bourne claimed, "by reason of the unique liberty of opportunity and traditional isolation for which she seems to stand, can lead in this cosmopolitan enterprise." For Bourne, exceptionalism could lead to transnationalism; the two were not antithetical, as so much of Tyrrell's article would seem to suggest.²¹

Tyrrell recognizes this possibility but holds back from it. He sees "how closely 'internationalism' has been linked to American exceptionalism through concepts of cultural expansionism." Yet Tyrrell cannot quite bring himself to accept that transnationalism may depend on exceptionalism. "In such judgments," he writes, "Bourne's cosmopolitan vision becomes confused and scarcely rises above the level of cultural imperialism advocated by some missionary groups and their

¹⁹ Agnew, *United States in the World-Economy*, 15; Ross, *Origins of American Social Science*.

²⁰ Tyrrell, "American Exceptionalism," 1049.

²¹ Randolph S. Bourne, *War and the Intellectuals: Essays by Randolph S. Bourne, 1915–1919*, Carl Resek, ed. (New York, 1964), 114, 120–21.

supporters.”²² Why should we assume that Bourne was confused? Transnationalism may well be a form of imperialism; the transnational world may well emerge from such unlovely phenomena as American power and American exceptionalism. Tyrrell flinches from such a conclusion. He delicately leaves “the close association” of exceptionalism and internationalism as an example of paradox and “irony.”²³ But there may be nothing paradoxical or ironical about the situation at all.

Perhaps, then, we should continue to study America in a comparative context, however difficult that may be. We do not want to resurrect the chauvinistic exceptionalism Tyrrell describes. But we should be prepared, at least, to consider the possibilities of American difference in an internationalizing world. Despite his earlier strictures, Tyrrell seems to think so. “If a history of transnationalism is to have any meaning, therefore, it must,” he writes, “explore the complex dialectic between exceptionalism and internationalism.”²⁴ This will be hard to do, if we believe one side of the dialectic does not exist and should not be studied.

WHAT, THEN, OF TRANSNATIONAL HISTORY? Certainly, there is no reason to reject it. As Tyrrell makes clear, there are several good reasons to undertake transnational projects. But what exactly is “transnational history”? Tyrrell outlines at least three varieties, each with its own implications.

The first variety is a history of “international connections.” This is mainly what Tyrrell offers toward the end of his article—instances of the impact of international trade, migration, and reform movements on the United States. There are other such connections, among them religions and multinational corporations, which historians can profitably study. Not especially novel, the history of “international connections” encourages historians to take into account transnational influences and institutions; it does not appear to demand a radical reorientation of historiography.

A second form of transnational history, only glimpsed in Tyrrell’s article, is far more revolutionary. “European expansionism,” he writes, “has gradually created a global economy and is today in the process of creating a global society as well.”²⁵ Here is a summons for historians to produce not just an improved version of an old, national story but a new narrative altogether. The focus shifts from how international factors shape nations to how nations and other forces will create the “global society.” This is an especially challenging task because it demands some confidence about the direction of humankind. Will we in fact create a “global society”? How do we define it? Presumably, it will be shaped by economic and media networks, by nation-states and natural resources. But will it be characterized by a common culture, a common sense of identity and purpose? Far more than the history of “international connections,” the story of “global society”

²² Tyrrell, “American Exceptionalism,” 1052–53.

²³ Tyrrell, “American Exceptionalism,” 1052.

²⁴ Tyrrell, “American Exceptionalism,” 1053.

²⁵ Tyrrell, “American Exceptionalism,” 1044.

demands that historians reorient their work—away from nation-centered projects and toward an uncertain future.

The third kind of transnational history focuses on the environment. Tyrrell does not have much opportunity to develop his views on environmental history, which is unfortunate because in some respects it represents the most radical departure from traditional historiography. As he notes, environmental history, concerned with ecological transformations, is impatient with national boundaries. But environmental history truly commands our attention because it can, to use a term Tyrrell favors, "contextualize" human beings. Traditionally, scholars have placed the thought and actions of human beings at the center of written history. But environmental history, focused on nature, can reduce men and women from stars to bit players. "Indeed, I have little to say about individual men and women," confessed the environmental historian William Cronon in his ecological study of Chicago and its hinterland. "The few who do show up in these pages are mainly merchants, who enter my narratives less because they are significant in their own right than because they exemplify so well the broader city-country connections I wish to trace." Environmental history suggests a true world history in which the planet itself, not humanity, drives the narrative.²⁶

OF COURSE, THIS CONCEPT MAY SEEM A BIT UNSETTLING. One of the best features of Tyrrell's article is that it forces us to consider what written history is all about. People? Nations? Global society? Nature? Over the last generation, we have tended to dodge the question by answering that history is about anything and everything we choose to consider. This catholicity, this pluralism, is one of the benefits of the historiographical revolution fought out in the last few decades. But Tyrrell reminds us that any answer is arbitrary, that historiography is constructed and even "artificial."²⁷

At some points, transnational history seems to threaten the pluralist bargain that underlies the variegated historical writing of our day. As we have seen, transnational history has difficulty coexisting with the traditional national histories; it also may challenge the array of newer fields gathered under the broad rubric of "social history." Although Tyrrell does not explore this possibility, he does deliver one enigmatic yet forceful blow. "The rise of historical specialization has shattered these confident [exceptionalist] assumptions," he declares, "but put nothing convincing in their place."²⁸ That is a hard judgment, but perhaps a necessary one for Tyrrell's transnational purposes. So much of recent historical writing has involved a turning inward, to private spaces, personal lives. Social history often invites us to measure experience on a small, intimate scale. Transnational history, on the other hand, asks us to turn outward, perhaps to the grand story of the making of "global society." Environmental history also raises potential problems for social history, which is ironic since the two seem to have a common appeal for academic liberals. But social history has been concerned

²⁶ William Cronon, *Nature's Metropolis: Chicago and the Great West* (New York, 1991), xv.

²⁷ Tyrrell, "American Exceptionalism," 1034.

²⁸ Tyrrell, "American Exceptionalism," 1031–32.

above all with people; it has insisted on the worth and interest of any human being, regardless of his or her wealth and power. Environmental history, as we have noted, has the potential to “contextualize” human beings and shift attention away from them. It may be that the newer social history, far more than national history or exceptionalism, will be an obstacle to the transnational history. Perhaps there is basis for reconciliation. But it is interesting that Tyrrell finds “nothing convincing” in the work of the last generation.

Transnational history does share at least one quality with recent historical writing—an unease with nationalism and the nation-state. This is hardly surprising. Historians of the United States, like many Western intellectuals, no longer see liberating possibilities in nationalism. In the United States, after the failures of the Vietnam War, the “Great Society,” and the “Reagan Revolution,” the nation-state does not seem so appealing to academic liberals and radicals. In domestic affairs, it appears to have few answers to people’s problems; in foreign affairs, it appears to have the wrong answers. Some historians, like E. J. Hobsbawm, look to the demise of nations and nationalism around the world. Within American historiography, there is a reluctance to focus on the nation-state. This tendency is particularly apparent in our treatment of the Civil War, which looms more as a conflict over slavery and less as a struggle over American nationalism. Abraham Lincoln and his almost mystical sense of nationalism have become remote. The “new American history” of the last generation has had incalculable benefits. But it has tended to evade the hard questions wrapped up in the triumph of American nationalism. Transnational history, for all its advantages, seems to offer another means of avoiding the realities of nationalism and national power. The potential costs include an incomplete history and an estrangement from our audiences, which, at least in the United States, still seem intensely nationalistic.²⁹

There is another cost to the new transnational history as described by Tyrrell. Granting that the choice of a focus for our histories is arbitrary, we can still ask how much we should bend the discipline of history to suit the needs of internationalism. There is a nagging sense here that the historical record can be manipulated in the service of the “new transnational history.” Noting that “exceptionalism could still be rescued at the level of consciousness,” Tyrrell writes, “In part, this pitfall might be avoided by historicizing American exceptionalism as a set of ideas in cultural history.”³⁰ Why is the discovery of any apparent fact a “pitfall”? We know that “facts” and “truth” are slippery, conditioned things. We know that historiography is the product of human beings. But, presumably, historians have a commitment to facing up to what they find. One wonders about that commitment, however, when the results of research are treated as a “pitfall,” when work on national differences must be halted because readers might draw the wrong exceptionalist conclusions, when the writings of Hartz and others are reshaped into an exceptionalist straw man, and when apologies must be offered for finding evidence of exceptionalism. Important as the mission of transnational history may be, it is not more important than the discipline of history itself.

²⁹ E. J. Hobsbawm, *Nations and Nationalism since 1780: Programme, Myth, Reality* (Cambridge, 1990); Eric Foner, ed., *The New American History* (Philadelphia, 1990).

³⁰ Tyrrell, “American Exceptionalism,” 1049.

The "new transnational history" offers a sensible, sometimes inspired intellectual agenda. But in Tyrrell's form, it comes at too high a price. That price does not have to be paid, however. Without indulging in national chauvinism or sacrificing the virtues of transnationalism, we can still consider questions of national difference, still confront the particular problem of the United States in world history, still deal with facts and evidence. And we can still maintain our ties to earlier historians who struggled with the problems of nations and nationalism in an interconnected world.

Some of those historians provide a useful example. Hartz, Potter, and Woodward at least showed how consideration of the peculiar nature of the United States could lead, even in the charged atmosphere of the cold war, not to parochial national history or chauvinistic nationalism but to a critical engagement with national feeling and the nation itself. However imperfect their comparative method, these historians treated the United States from an international perspective. They tried to confront head on both the unpleasant realities and the liberating possibilities of nationalism. Attempting to come to terms with Lincoln and American nationalism, Potter offered the best illustration. He had few illusions about nineteenth-century nationalism: "Abraham Lincoln lived in a century which was marked pre-eminently by extreme, jingoistic, saber-rattling nationalism—nationalism in an advanced stage of egocentrism and resorting to every device of deceit, repression, and mass violence to achieve its ends." Potter also had few illusions about Lincoln's United States: "He lived in a country acutely convinced of its own national superiority." But Potter could note "one of the most striking features of Lincoln's greatness that . . . he fused the cause of nationalism with that of freedom. The other great nation-builders of the century—Napoleon, Cavour, Bismarck—sacrificed mankind at large for the enhancement of French, or Italian, or German power. Amid such developments it is a happy reflection for the United States that the most distinctively American figure in the national history, and the major leader in the consolidation of American national power, bore a significance least narrowly American."³¹

Potter, Hartz, and Woodward had their own evasions and illusions, to be sure. We can no longer have Potter's confidence in the concept of a unified national character. We cannot share Hartz's belief in an enduring national liberal consensus. We cannot, like Potter in *The Impending Crisis* or Woodward in *Origins of the New South*, write histories centered so fully on public life, electoral politics, and white men.³² But these historians are still useful guides as we move up to the "new transnational history." It would be a shame to seize the moral and intellectual high ground only to substitute our own evasions and illusions for theirs.

³¹ Potter, *South and the Sectional Conflict*, 176.

³² David M. Potter, *The Impending Crisis, 1848–1861* (New York, 1976); C. Vann Woodward, *Origins of the New South, 1877–1913* (Baton Rouge, La., 1951)

MICHAEL MCGERR BROADLY ACCEPTS the validity of a transnational historical project outlined in my essay. Space limitations prevent a comprehensive response, but our most genuine disagreement appears to be over the influence of American exceptionalism and its implications for research. I say “genuine” with some uncertainty, since after receiving McGerr’s “reading” of my argument, I wondered whether there was more to deconstructionist notions on textuality than I had previously understood.¹

Has recent American historiography been exceptionalist? I did not depict a full-blown exceptionalism in recent writing but stated that “many . . . eschew exceptionalism” and pointed only to evidence of a “modest revival” of the idea. McGerr and I am on the same side also insofar as he appears to be “against exceptionalism.”² Yet all three published critiques of Sean Wilentz’s splendid essay of that title, mentioned in my footnotes, utilize elements of the exceptionalist heritage. Salvatore deploys Tocqueville to argue for a broad ideological consensus; Sapolsky even uses, with some trepidation, the word “exceptionalism” to describe class relations from the 1890s on; Hanagan emphasizes the old question of the weakness of socialism in the United States versus a variety of European countries. In another article, Oestreicher concludes that it is the absence of “class consciousness” in the face of structural similarities of class that “has been most exceptional about modern American politics, compared to west Europe.” This is a more careful use of exceptionalist ideas than hitherto and is informed by the comparative method, which I have simultaneously admired for its strengths and criticized for its limitations. Highly qualified though it is, this approach to national difference still retains important links with exceptionalism through its affirmation of American divergence from European patterns of class relations and ideology.³

Were the post-progressives exceptionalists? Have I concocted a “straw man”? McGerr is right that the post-progressives were not uncritical scholars. Yet it is not necessary to proclaim “national superiority” to be an exceptionalist. The denunciation of the United States as the source of all evil by many anti-Americans abroad and by some New Leftists at home attests to this. Even Seymour Martin Lipset, whom McGerr concedes as an exceptionalist, does not neglect the negative aspects of an American national character. There is also a difference in methodology here between McGerr and me. I have not sought to review the complete *oeuvre* of the post-progressive generation that includes Hartz, Potter, and Boors-

¹ Greg Manning explains this position: “There is ‘no text itself’ beyond the act of reading.” Manning, “A Proliferation of Hypotheses: Postmodernism and Literary Studies,” *Australasian Journal of American Studies*, 10 (July 1991): 23.

² Ian Tyrrell, “American Exceptionalism in an Age of International History,” *AHR*, 96 (October 1991): 1032; Michael McGerr, “The Price of the ‘New Transnational History,’” *AHR*, 96 (October 1991): 1057.

³ See Tyrrell, “American Exceptionalism,” n. 4; Sean Wilentz, “Against Exceptionalism: Class Consciousness and the American Labor Movement, 1790–1920,” *International Labor and Working Class History*, 26 (Fall 1984): 1–24; Nick Salvatore, Michael Hanagan, and Steven Sapolsky, *ibid.*, pp. 25–36; and *ibid.*, 27 (Spring 1985): 35–38; Richard Oestreicher, “Urban Working-Class Political Behavior and Theories of American Electoral Politics, 1870–1940,” *Journal of American History*, 74 (March 1988): 1283.

tin, the three scholars against whom I and others have taken principal aim.⁴ I am using the key texts of these writers, the ones that served as exemplars for American historiography. It is possible to admire, for example, Potter's major posthumously published work on the sectional conflict without having to endorse his much more influential 1950s account of American abundance. Similarly, Hartz's *Founding of New Societies* extends the liberal fragment theory in interesting comparative directions that ought to be followed up today in a more nuanced examination of differences in settler societies, but this is not the book cited and confronted by a generation of American historians. *The Liberal Tradition*, which, as Dorothy Ross correctly explains, exemplifies modern liberal exceptionalism, is widely used.⁵

McGerr trivializes the issue, raised forcefully by postmodern literary theory, of the production and dissemination of texts in discourse when he conjures up the image of a hapless historian daunted by tall stacks of nation-centered books. Volume does matter in discourse, especially when works by prestigious and able figures like Potter appear almost simultaneously, when research money and time is limited, when increasing specialization makes grasping the connecting assumptions I treat difficult.

It is in this context that I put difference. It would be absurd for a historian, even one of transnationalist sympathies, to disparage national difference as "unacceptable," any more than other differences. Different forms of historical writing can "coexist," though I would like to see national differences "de-emphasized" (I would prefer to say "historicized") by devoting a greater proportion of scholarly endeavor to transnational analysis.⁶ "Splitting" as well as "lumping," particularism as well as universalism, are our stocks in trade. It is the quantity of research already focused on articulating a specific national difference that I noted. I suspect that American difference is already too firmly embedded in historiography to be obliterated by a transnational project.

Nor do I expect comparative history to be abandoned. A great deal more could be done, however, to detach comparative history from the question of national difference. For example, studies of comparable regions or towns within one country such as that done by Daniel Walkowitz could be fruitfully explored.⁷ Also, in the international arena, studies that proceed from awareness of some problem in a historiography other than the American would be useful. Greater attention could be given to comparisons with other new world societies, and comparative

⁴ McGerr, "Price," 1066, 1057; Woodward is a somewhat different case that I treated only in passing. Woodward summed up the previous historiography emphasizing free space while he simultaneously attempted to historicize that tradition. My reference was intended to document the persistence of Turnerian concepts, not to deny Woodward's historicization. See also Ian Tyrrell, *The Absent Marx: Class Analysis and Liberal History in Twentieth Century America* (Westport, Conn., 1986), chap. 3, where the diversity of post-progressive scholarship is recognized.

⁵ Dorothy Ross, *The Origins of American Social Science* (Cambridge, 1991), xvii.

⁶ Compare McGerr, "Price," 1056, 1061.

⁷ Daniel J. Walkowitz, *Worker City, Company Town: Iron and Cotton-Worker Protest in Troy and Cohoes, New York, 1855–85* (Urbana, Ill., 1979).

analysis could be “combined” with transnational analysis as I did in *Woman’s World/Woman’s Empire*.⁸

McGerr thinks he detects a contradiction in my alleged reification of exceptionalism. But he did not mention that I showed other strains in American historiography and culture, such as regionalism and transnationalism, that have waxed and waned, contesting the power of exceptionalism, which I also depicted as uneven in its operation over time. Moreover, I have elsewhere emphasized that the liberal tradition has not been unchanging but has mutated repeatedly to take new shape and force.⁹

It is not fatal that I show exceptionalist ideas to be closely linked to transnationalism. Exceptionalism is but one source of internationalist ideas; Marxism, for example, is another. Counter-hegemonic ideologies among the working classes may provide others, as my article suggests. Because all ideas are rooted in social contexts, the ambiguities of, and contradictions between, “genuine” internationalism and exceptionalism are inescapable realities. Transnationalism may be a form of imperialism, too, as McGerr says, but the United States is not the only source of that “unlovely” phenomenon. In *Woman’s World/Woman’s Empire*, I show how ideas of international cooperation and organization influenced turn-of-the-century American women temperance reformers and how the imperialism that influenced their internationalism was an Anglo-American kind.¹⁰

Detecting an exceptionalist ideology in the United States is not the end of the story; we do not simply confront this as an unpleasant “fact”; it has to be compared with other cases of national hubris. So far, this has not been done in a systematic way, but there is tantalizing evidence for making comparisons and links between many exceptionalisms. (Bernard Porter, for example, argues that people in Victorian Britain felt a strong sense of superiority rooted in material progress and “free” institutions).¹¹ An issue for future research: did U.S. exceptionalism’s anticipation of American political and economic hegemony distinguish it from all other ideologies of national superiority? If so, this could possibly be construed as evidence for McGerr’s viewpoint. The question is open, although I doubt the answer is yes. As Raphael Samuel pointed out, John Foxe’s *Book of Martyrs* was “for some two centuries [ca. 1570s–1770s] the most widely read text on English history” and has been depicted as “the substantial basis” for “the idea of England as an ‘elect nation’ and for the Whig-liberal view of the national past.”¹² The French and Germans harbored their own exclusivist notions that have affected the writing of national history. The real “pitfalls” in any treatment of exceptionalist ideology would be twofold: using exceptionalist consciousness as a surrogate for analysis of social structure and avoiding discussion of alternative ideologies

⁸ Ian Tyrrell, *Woman’s World/Woman’s Empire: The Woman’s Christian Temperance Union in International Perspective, 1880–1930* (Chapel Hill, N.C., 1991), 7.

⁹ McGerr, “Price,” 1062; Tyrrell, *Absent Marx*.

¹⁰ McGerr, “Price,” 1064.

¹¹ Bernard Porter, *Britain, Europe and the World 1850–1982: Delusions of Imperial Grandeur* (London, 1983), 1.

¹² Raphael Samuel, “Grand Narratives,” *History Workshop*, 29 (Spring 1990): 124; McGerr, “Price,” 1066.

that have contested or questioned exceptionalism.¹³ I discuss some of these alternatives in my section on international movements and organizations.

McGerr raises three crucial issues on which there is common ground of concern: nationalism, the audience of historians, and the role of people in history. I am most alarmed by McGerr's suggestion that transnational history might be remote from a humanistic history. The transnational history of international movements such as that of the Woman's Christian Temperance Union does not have to neglect people. This version of transnationalism is a form of the new social history, not a repudiation of it. (When I stated that the newer social history—which I have practiced myself—had put “nothing convincing” in place of the exceptionalist vision, I was clearly referring to the question of synthesis). The humanistic perspective also troubles me when I consider world-systems theory, where hegemonies rise and fall often coldly disembodied from the social groups that must articulate such notions in consciousness. This defect has enabled nimble wits like Simon Schama to have great fun at the expense of world-systems concepts and to draw attention away by the force of their rhetoric from important work being done on changing structures of international hegemony.¹⁴

Environmental history, too, probably neglects people in some instances, and I regret this, but it is not a matter of how many individuals are named in a text.¹⁵ Environmental history must show the connections of large groups of people with their environment collectively, how they have both transformed and been limited by the environment. Donald Worster, in two outstanding books, has done just that. He has also managed to combine his emphasis on specific regions of the United States with a strong sense of transnational processes in environmental history.¹⁶

If transnational history could grasp the importance of incorporating in a “grand narrative” a humanistic perspective that includes a sense of people both changing and being changed by history, this might avoid the possibility of a loss of audience. A sense of belonging to place or group is a vital human need, but it does not necessarily mean only an exclusive national identity. Audiences want many things. Their fragmentation is reflected in the history being written. Some favor an intensely nationalistic perspective, others a parochial evocation of their own regions, localities, or groups; still others will be interested in international connections and the meaning such history has for the world conjuncture of the 1990s.

It has almost been worthwhile writing the article to see McGerr highlight the importance of nationalism in the historiographical agenda. The past generation of historians has unduly neglected this theme, particularly in relation to the Civil War. Yet one would not learn from McGerr that I wrote of my approach: “This does not mean that these national perspectives can be ignored. It does mean that

¹³ Compare McGerr, “Price,” 1063, 1066.

¹⁴ Simon Schama, *The Embarrassment of Riches: An Interpretation of Dutch Culture in the Golden Age* (New York, 1987), 6; compare Giovanni Arrighi, “The Three Hegemonies of Historical Capitalism,” *Review*, 13 (Summer 1990): 365–408; Tyrrell, “American Exceptionalism,” 1032.

¹⁵ Compare McGerr, “Price,” 1065.

¹⁶ Donald Worster, *Dust Bowl: The Southern Plains in the 1930s* (New York, 1979); *Rivers of Empire: Water, Aridity, and the Growth of the American West* (New York, 1985).

these national perspectives must be historicized and relativized . . . in terms of a simultaneous consideration of differing geographical scales—the local, the national, and the transnational.”¹⁷ One of the benefits of an awareness of international factors and approaches is to draw attention to nationalism as part of a larger synthesis that I indicate in my three-tier scheme. The nationalism I want to see depicted would be constantly changing and contingent. This awareness of nationalism’s importance is tied to a broader historiographical reorientation that includes Braudel’s final, enigmatic work. Braudel’s espousal of national identity is perfectly compatible with the model that I offer—indeed, more so than the earlier Braudel—but McGerr does not discuss this aspect of my work. It is also interesting that, although Braudel turned to the national terrain, *The Identity of France* is organized on the same levels of environment, social structure, and political events as *The Mediterranean*. After stressing that “regions, provinces, *pays* . . . long maintained and still do maintain a significant degree of autonomy,” Braudel enunciated his scheme linking French history with “world history.” “The *longue durée* then (first and indeed foremost), the hexagon [the present-day territory of France], Europe, the world—these are the dimensions of space and time with which I shall be working.”¹⁸

McGerr does not help advance the study of nationalism or resolve the issues of exceptionalism in dispute, however, by concluding with Potter’s conflation of nationalism and “freedom” in Lincoln. This passage illustrates Potter’s thoughtful use of comparative history and his awareness of the darker side of nationalism, but what is the interpretation Potter divines in the complex interaction of national consolidation, state building, and developing national ideologies in the nineteenth century? He stresses ideological meaning rather than social structure. Of all the variants examined, only American nationalism is endowed, through the rhetoric of the great emancipator, with a “significant moral purpose” in world history and set against the “group egocentrism or chauvinism” in European examples.¹⁹ Sounds just a bit like exceptionalism after all.

¹⁷ Tyrrell, “American Exceptionalism,” 1033.

¹⁸ Fernand Braudel, *L’Identité de la France: Espace et histoire* (Paris, 1986), 9–10, 14–15, 19.

¹⁹ David Potter, “Civil War,” in C. Vann Woodward, ed., *A Comparative Approach to American History* (New York, 1968), 154, 155, 156.

The Warlord: Twentieth-Century Chinese Understandings of Violence, Militarism, and Imperialism

ARTHUR WALDRON

IN THE CHINESE VOCABULARY, “warlord” (*junfa*) is what Raymond Williams calls a “keyword”: a term whose history and usage reveal how certain fundamental issues are understood.¹ This essay will examine the word and the idea of the “warlord” in China from the late 1910s, when it appears for the first time, to the present and will pay particular attention to what the use of the concept reveals about changing Chinese understandings of larger social and economic questions, specifically those of “militarism” and “imperialism.”

The term *junfa* is a relatively new one in Chinese. Apparently never used in the nineteenth or early twentieth centuries, it gained currency in the early 1920s and had come to dominate political discussion by the end of the decade. Many people believe that it is a native Chinese term for a characteristically Chinese pattern of politics. Indeed, according to the *Oxford English Dictionary*, one sense of the English word “warlord” derives from the Chinese *junfa*.²

Plausible as such a derivation may be, I will argue that it is incorrect and that, far from being an indigenous Chinese category, the idea of the “Chinese warlord” or *junfa* is borrowed, ultimately, from Europe. But more important than this will be the argument that the adoption of the concept in Chinese marks a fundamental transition in the Chinese understanding of violence. Talk about “warlords” usually turns fairly quickly away from the individuals themselves to broader questions about their social and economic bases, both domestic and foreign. And these, whether we are aware of it or not, bring us to the issues that in the West have been debated under the broad heading of “militarism.” Thus the major purpose of this essay is to show how the turn-of-the-century Western debate about violence, militarism, and imperialism entered China, how it was assimilated, and not least, how it was modified to suit Chinese conditions.

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¹ Raymond Williams, *Keywords: A Vocabulary of Culture and Society*, rev. and expanded edn. (London, 1984).

² “[A] military commander who has a regional power base and rules independently of the central government, esp. in the period 1916–28”; R. W. Burchfield, ed., *A Supplement to the Oxford English Dictionary* (Oxford, 1986), 4: 1217–18.

Since the “warlord” is so often thought of as a Chinese “ideal type,” perhaps I should begin with Max Weber’s observation in *The Religion of China* that China lacked “the military charisma of the warlord.”³ Weber did not mean, of course, that Chinese history lacked either wars or warriors; rather, he was pointing out a difference between China and the West in the way violence has fit into the larger structures of society and culture. Subsequent work confirms his insight. The origin of the state in the West is generally thought to have involved violence; in China, by contrast, recent interpretations argue that ritual was the key.⁴ Although China has produced great generals such as Zhuge Liang (A.D. 181–234) and Yue Fei (1103–1142), their classical cults differ in important ways from the Western-style celebration of heroic martialism, for example, of Alexander the Great.⁵ The reasons are both philosophical and social. Early Chinese thinkers tended not to separate military issues from other concerns but rather to merge them into larger questions of society and order in which primary attention was given to the search for moral means to alleviate conflict with the least expenditure of energy.⁶ In the Warring States period and early imperial China, “the man . . . of combat was a suspect and dangerous figure who fell outside of human society.”⁷ The greatest Chinese military theorists scorned the use of violence, although their Western equivalents saw it as absolutely central in war. Carl von Clausewitz (1780–1831) in *On War* called war “an act of force” and used the word *Gewalt* (power, force) eight times in the second and third paragraphs of Chapter 1 alone, while Sun Zi (fl. B.C. 500) by contrast used the word *li* (force) but nine times in all of the *Bingfa* (Art of War).⁸

The social organization of violence in China has clearly reflected these deep cultural differences. Armies there have generally been bureaucratic, with high command positions going to civilian officials. In the Ming dynasty (1368–1644), for example, Wang Yue (1426–1499), famous commander of daring mounted raids deep into the steppe, was a Confucian scholar who had earned the coveted civil *jinshi* degree in 1451 and not its far-less-honored military counterpart.⁹ Except in periods of dynastic transition, warlord-like figures have been rare in China, and for these (the *jiedushi* of the late Tang dynasty, for example), there is no general term in the traditional historical vocabulary. As Mary Wright has pointed out, even in the Qing dynasty (1644–1912), when huge personal forces were raised to combat the great rebellions, they functioned as “regionally based national armies.” Far from being proto-warlords, their leaders placed national

³ Max Weber, *The Religion of China: Confucianism and Taoism*, Hans H. Gerth, trans. and ed., C. K. Yang, intro. (New York, 1951), 30.

⁴ See Kwang-chih Chang, *Art, Myth, and Ritual: The Path to Political Authority in Ancient China* (Cambridge, Mass., 1983).

⁵ For the pervasive influence of the Alexandrine model in the West, see John Keegan, *The Mask of Command* (New York, 1987).

⁶ See Christopher C. Rand, “Chinese Military Thought and Philosophical Taoism,” *Monumenta serica*, 34 (1979–80): 171–218; “The Role of Military Thought in Early Chinese Intellectual History” (Ph.D. dissertation, Harvard University, 1977).

⁷ Mark Edward Lewis, *Sanctioned Violence in Early China* (Albany, N.Y., 1990), 225.

⁸ Carl von Clausewitz, *On War*, indexed edn., Michael Howard and Peter Paret, ed. and trans. (Princeton, N.J., 1984), 75; Sun-tzu on the Art of War—*The Oldest Military Treatise in the World*, Lionel Giles, ed. and trans. (London, 1910).

⁹ *Mingshi* (Beijing, 1974), 171: 4570–77.

higher than regional interests, often incurring the ire of local elites, as Zeng Guofan (1811–1872) did in Jiangxi.¹⁰

But, after the abdication of the Qing in 1912, the situation began to change. In 1916, provincial military commanders acquired the title *dujun*, literally, “supervisor of military affairs.” To a Chinese, even this term still expressed an understanding of the relationship of the military to politics that was rooted in the dynastic past: a *dujun* was a supervisor and by no means an independent “lord.” Understanding this, most *dujuns* sought legitimacy not from their military charisma, if any, but rather from pretended civilian and scholarly virtues. Modern writers accustomed to speaking of *junfa* in the 1910s will be surprised by how long the word *dujun* continued to be used in China: well into what today is called the “warlord” period. When Zhang Xun (1854–1923) attempted a restoration of the Manchus in July of 1917, for example, the progressive journal *Xinqingnian* framed its comment in terms of the *dujun* system. A six-page article denounced the machinations of the northern military leaders in damning terms—but without using the word *junfa*.¹¹ Likewise, the denunciation by Liang Shuming (1893–1988) of the abuses of Duan Qirui’s (1865–1936) forces in Hunan does not use the term, although his biographer speaks of “warlord troops” when describing what Liang wrote.¹² Well into the 1920s, critics directed their ire against *dujuns*: for those opposed to military depredations, “feidu caibing” (abolish the *dujuns* and reduce the troops) was the rallying cry.¹³

IN THE WEST, THE CHINESE WORD *dujun*—or *tuchün* in its more familiar Wade-Giles form—was also widely used. But it became current at a time when the term warlord was also common. “Warlord” had entered English vocabulary in the nineteenth century: Ralph Waldo Emerson used it in a passage that described how, in the English aristocracy, “piracy and war gave place to trade, politics, and letters; the war-lord to the law-lord; the law-lord to the merchant and the mill-owner; but the privilege was kept, whilst the means of obtaining it were changed.”¹⁴ At the same time in Germany, the old term *Kriegsherr* was coming into new currency in connection with debates about the constitutional limits of monarchical authority in Prussia. Both as king of Prussia and German emperor, William I claimed that being *Oberster Kriegsherr* gave him absolute and direct authority over the military without parliamentary or ministerial rights of co-determination.¹⁵

¹⁰ Mary C. Wright, *The Last Stand of Chinese Conservatism* (Stanford, Calif., 1957), 221, emphasis added. David Pong, “The Income and Military Expenditure of Kiangsi Province in the Last Years (1860–1864) of the Taiping Rebellion,” *Journal of Asian Studies*, 26 (November 1966): 49–65, cited in Harold Z. Schiffrin, “Military and Politics in China: Is the Warlord Model Pertinent?” *Asia Quarterly*, 3 (1975): 196–97.

¹¹ “Dujun chengbing yu fubi,” *Xinqingnian*, 3 (August 1917): 561–67.

¹² Liang Shuming, *Shuming saqian wenlu* (1924; rpt. edn., Taipei, 1972), 39–56, quoted in Guy Alitto, *The Last Confucian: Liang Shu-ming and the Chinese Dilemma of Modernity* (Berkeley, Calif., 1979), 60 n. 56.

¹³ He Xiya, “Jiazi dazhanhou quanguo jundui zhi diaocha,” *Dongfang zazhi*, 22 (1925): 103–04.

¹⁴ Ralph Waldo Emerson, *English Traits* (1856), in Charles W. Eliot, ed., *The Harvard Classics* (New York, 1909), 5: 419; *The Compact Edition of the Oxford English Dictionary* (Oxford, 1971), 2: 3688.

¹⁵ See A. Lawrence Lowell, *Governments and Parties in Continental Europe*, 2 vols. (London, 1896), 1: 272.

Nineteenth-century popular culture provided images for the words. The first embryonic sketches of what would eventually become the full-blown Western warlord seem to have flowed from the pen of Honoré Daumier beginning in the 1850s (Figure 1).¹⁶ And there was the musical stage, in particular, Jacques Offenbach's operetta, *La Grande Duchesse de Gérolstein* (1867), whose London success stimulated Richard D'Oyly Carte to form his company. It portrayed the forces of a "joke German principality . . . led by a joke German general called Boum, as incapable as he was fearless."¹⁷ Applied to China, such images contributed to a stereotypical Western description of wars among the *dujuns*: mercenary in purpose, reliant on ritual rather than real battle, and preferring "silver bullets" (bribes) to the genuine article. Seen from the West, Chinese wars were "Celestial Opera Bouffe," in which "one wonders whether one has not, somehow or other, fallen into the middle of a new Gilbert and Sullivan opera where presently the painted general will elope with the painted Mimi, and the soldiers will all march in with parasols over their heads and sing the chorus."¹⁸

Such images of Western warfare perished on the western front. With the advent of World War I, German generals were no longer even remotely ridiculous. Considering the carnage, many asked and answered, as did Charles Edward Russell, "What man is to blame for all this? The War Lord with his overweening ambitions."¹⁹ Russell was referring to the kaiser himself, but the term quickly became generalized, and "warlords" became a sinister category (see Figure 2).²⁰ The term was applied to China as well, although still rather mockingly. Explaining the civil war of 1922, *The New York Times* wrote, "Each provincial Tuchun or Military Governor is a little or big war lord with his own army and his own laws; and his regard for the Peking government is proportioned inversely to the size of his army and his distance from the capital."²¹

Bloody internecine fighting in 1924, however, began to challenge the Western assumption that Chinese warfare was all an elaborate joke. Unlike some of the earlier encounters among *dujuns*, which had to a degree been ritual affairs, the wars of 1924 were serious business. They began with a struggle between Jiangsu and Zhejiang for control of Shanghai, which expanded into a showdown in Beijing and the north as well, the Second Zhili-Fengtian War. All the rolling stock in north China was pressed into service to carry men and weapons to the front. Mines and barbed-wire protected entrenched positions; armored trains and

¹⁶ Among the examples found in Loys Delteil, *Le Peintre-Graveur illustré: The Graphic Works of Nineteenth and Twentieth-Century Artists: An Illustrated Catalog* (New York, 1969), vol. 29, Daumier, see in particular "Le Général Guilay taillant ses ennemis" (1859), vol. 17, no. 3172; "David et Goliath," vol. 16, no. 2521; "Tableau . . . de l'empire de Russie," vol. 16, no. 2522; "A L'Instar de Pantin," vol. 29, no. 3740.

¹⁷ Alistair Horne, *The Fall of Paris: The Siege and the Commune, 1870–1871* (Harmondsworth, 1981), 27–28.

¹⁸ Ethel Andrews Murphy, "Celestial Opera Bouffe," *Travel Magazine* (Floral Park, N.Y.), 40 (April 1923): 15.

¹⁹ Charles Edward Russell, "Who Made This War?" *Pearson's Magazine*, 32 (November 1914): 514.

²⁰ "It is not . . . universal training to defend the country that creates German militarism. It is the enormous body of officers . . . [who] constitute a military caste that is associated with the surviving institutions of autocracy and feudalism"; "The Progress of the World," in *The American Review of Reviews*, 50 (September 1914): 266; see also "The Lorelei" (Figure 3).

²¹ *New York Times*, December 31, 1922, viii.12/13, quoted in *The Compact Edition of the Oxford English Dictionary*, 2: 3688.



Tableau politique et social de l'empire de Russie

FIGURE 1: Two early works of Honoré Daumier, "Le Général Giulay taillant ses ennemis," 1859, and "Tableau politique et social de l'empire de Russie," 1854. Bibliothèque Nationale, Cat. H.D. 2992, 3019.



FIGURE 2: Prototype of Prussian warlord by Daumier, 1869, "A L'Instar de Pantin": the strangling of Baden. Bibliothèque Nationale, Cat. H.D. 3325.

military aircraft were employed, and artillery fire was punishing. Both sides used machine-gun corps to kill their own troops if they retreated without order.²² Casualties were extremely heavy. Economic, social, and political life was thrown into deep confusion.

Even foreign journalists recognized the change. In September 1924, "A

²² Hsi-sheng Ch'i, *Warlord Politics in China 1916-1928* (Stanford, Calif., 1976), 139-40.



THE LORELEI

"And this, with her wild, sweet singing,
The Lorelei has done."

From the *American* (Baltimore)

FIGURE 3: An American view of militarism. *The American Review of Reviews*, 50 (September 1914): 266.

Correspondent" set out from Shanghai, as many a treaty-port type had in years past, for a picturesque day watching the Chinese skirmish. But, when he and his unwilling driver drew near to the fighting, they found not diversion but corpses, casualties, and personal peril: "the appalling horrors of war."²³

These wars, and even worse fighting in following years, prepared the way in China for the post-World War I European understanding of war and European

²³ "Shanghai Battle Sketches," *The Living Age*, 323, no. 4191 (November 1, 1924): 235–39.

conventions for depicting it. According to the Europeans, violence could be either meaningless, as in the warfare of the trenches, or transformative and redemptive, as in the revolutionary wars that followed. The process is clearly evident in the increasing displacement of the word *dujun* by *junfa*, until, by the end of the 1920s, the new term, and the new way of thinking that came with it, became virtually standard.²⁴

The first Chinese to use the new word was probably Chen Duxiu (1879–1942), who published a paragraph titled “Dao junfa” (Down with warlords) on December 28, 1918, in *Meizhou pinglun* (Weekly Critic), a journal he had founded in collaboration with Li Dazhao (1889–1927).²⁵ Although rooted in a leftist understanding of events in China (Chen and Li were early Marxists), the term gained wider currency: at the beginning of the First Zhili-Fengtian War (April 28–May 4, 1922), the rival commanders Wu Peifu (1874–1939) and Zhang Zuolin (1872–1928) denounced one another as *junfa*.²⁶ In July 1922, the Second Congress of the Chinese Communist party explained the new concept. A resolution stated that, after World War I, European capital needed new outlets; this would intensify competition among imperialists, and foreign powers would therefore attempt to bolster their positions in China by sponsoring local warlords. The resolution called upon the Chinese people to “overthrow the feudal warlords and stop civil wars, so that internal peace can be established within China.”²⁷

In 1924, the term was also used by Zhang Junmai (1886–1969) in the “Six Lectures on Internal War,” which he presented to the National Political University in Shanghai. Zhang’s analysis was rather different from Chen’s and that of the Communists: he focused on the military basis of the power of the *junfa* and never mentioned the idea that they might be products of capitalism and imperialism. Their cure, he argued, was the creation of popularly based militias.²⁸

The term *junfa* also began to be used by Sun Yat-sen (1866–1925). For much of his career, Sun had based his hopes for power not only on his political program but also on military maneuvers, for example, in his intrigues with the American would-be military hero Homer Lea and his attempts to ally with various leading militarists.²⁹ But, by late 1924, Sun and his Guomindang party were changing their approach. Their propaganda increasingly stressed anti-militarism and anti-imperialism, and, when Sun lectured on the *Sanminzhuyi*, he became “more militantly nationalistic than before.” The northern expedition proclaimed on September 18, 1924, was directed “not only against [Cao Kun] and [Wu Peifu], but against all warlords and against the imperialism which supported them. Once imperialism had been overthrown, China would escape its position as a semicolon and create a free and independent state.” Such analysis was new for Sun.³⁰

²⁴ Jerome Ch’en, *The Military-Gentry Coalition: China under the Warlords* (Toronto, 1979), 2.

²⁵ Chen Duxiu, “Dao junfa,” in *Chen Duxiu wenzhang xuanbian* (Beijing, 1984), 1: 312.

²⁶ *Xinhaigeming yanjiulunji*, 2: 146, 160, cited in Ch’en, *Military-Gentry Coalition*, 2.

²⁷ Second Congress, held in Shanghai July 16–23, 1922. Wang Jianmin, *Zhongguo Gongchandang Shigao* (Taipei, n.d.), 1: 58–59, trans. in Dun J. Li, *Modern China: From Mandarin to Commissar* (New York, 1978), 197–98.

²⁸ Zhang Junmai, *Guonei zhanzheng liujiang* (Shanghai, 1924).

²⁹ See Eugene Anschel, *Homer Lea, Sun Yat-sen, and the Chinese Revolution* (New York, 1984); C. Martin Wilbur, *Sun Yat-sen: Frustrated Patriot* (New York, 1976).

³⁰ Wilbur, *Sun Yat-sen*, 34–35, 257–58. The proclamation of the northern expedition of 1924 is

By 1926, the word *junfa* was common enough to prompt discussion in the press of what it meant and thus, by implication, of how the military and political situation in China should be understood. The word turned out to mean different, and not always compatible, things to different people. One question concerned the social base of “warlords”: were they figures who ultimately derived their power from control of the means of coercion and therefore had little connection with the rest of society? Or were they closely tied to capitalists, the gentry, or some other class? Second, were they old or new? Had “warlords” always existed in China? Or were they an unprecedented phenomenon, perhaps created by internal and external changes over the preceding century?³¹

In February 1926, an article appeared called “What Is a *Junfa*?” by Wang Jizhan. Wang argued that “warlordism” was in fact traditional; during transitional periods such as the Warring States or the Three Kingdoms, it was common for soldiers to base themselves in parts of China while contending for control of the whole.³² But Gao Yihan, a political scientist from Peking University who published his analysis of the *junfa* in the same journal, disagreed and pointed out the importance of the issue to all who hoped for revolutionary change. He argued that if “warlordism” was simply a standard aspect of transition between dynasties, it followed that the present period of disorder would likewise be followed by yet another traditional-style Chinese regime. Since that was undesirable, the key question became how to end a period of “warlordism” in such a way that a revolutionary new regime, and not simply another dynasty, would emerge. Many revolutionaries had been trying to work with progressive warlords to promote change. Gao argued that this approach could never succeed. What was needed was a new kind of military man: someone who was a master of the techniques of warfare but a revolutionary and not a *junfa* at heart. Gao believed that Chiang Kai-shek (Jiang Jieshi, 1887–1975), just then taking center stage in China, was such a man: a “soldier but not a warlord” (*jun er fei fa*).³³

Gao’s article elicited two detailed responses from Wu Zhihui (1864–1953) who was, like him, a revolutionary but who nevertheless disagreed. Wu found Gao’s categories too exclusive: the *junfa* were not all as bad as he painted them nor was Chiang as good. Furthermore, to confront all the *junfa* directly was probably impossible militarily. It would be much wiser to try to work with them and “transform warlords into soldiers” (*hua fa wei jun*).³⁴

Gao’s idea that Chiang was the “soldier but not a warlord” who could accomplish this revolution seemed to be confirmed during the first stages of the

found in Luo Jialun, ed., *Geming wenxian* (Taipei, 1953-), 10: 1489–91; and in Milton J. Hsieh, *The Kuomintang: Selected Historical Documents 1894–1969* (New York, 1970), 87–90.

³¹ Western theorists faced the same problems. Karl Liebknecht (see below) could never decide whether militarism was a Prussian tradition that had survived into the modern age or the product of very recent industrialism.

³² Wang Jizhan, “Junfa shi shenmo dongxi?” *Jingbao*, February 20, 1926. I have not yet been able to consult the original text and rely on extensive quotations in Wu Zhihui, “Junfa wenti da Yihan xiansheng,” part 2, *Xiandai pinglun*, 3, 64 (1926): 225.

³³ Gao Yihan, “Pingmin geming de mudi yu shouduan,” *Xiandai pinglun*, 3, 53 (1926): 2–3.

³⁴ On Wu Zhihui, see Peter Zarrow, *Anarchism and Chinese Political Culture* (New York, 1990), 60–72 and *passim*.

successful Northern Expedition (1926–1928), as the Guomindang army surged north. The propaganda accompanying the campaign stressed that the “revolutionary” Guomindang armies differed in their essence from the “warlord” armies they were steadily defeating, and this view initially achieved wide credence.

After Chiang crushed the Communists in Shanghai in April 1927, however, his adversaries began to attach the label *junfa* to him and his regime as well, leading to division within the ranks of the triumphant revolution. Thus in October 1928, Mao Zedong called Chiang’s government “the new warlords of the Guomindang,” writing that, like the old warlords, the new ones could not hope to maintain peace, for their power remained based on “the comprador class in the cities and the landlord class in the countryside.” This view has remained standard in much Chinese scholarship ever since.³⁵

Such accusations outraged Guomindang loyalists. But both Communists and Nationalists had by this time adopted the new political language of invective, and, when Hu Hanmin (1879–1936) responded indignantly, it was from within an intellectual system that included the category of “warlord.” The problem was not its general validity but rather its misapplication. Real warlords, according to Hu, were only those who relied on such “counterrevolutionary forces” as corrupt officials, local bullies, oppressive gentry, and foreign imperialism.³⁶ In the space of seven years, the term *junfa* had come to define for many one of the fundamental problems of Chinese politics and society.

WHERE HAD THE TERM COME FROM? In its modern form, it was not Chinese in origin, although the characters of which it is composed are both ancient: *jun*, as defined by Giles, is “an army, according to the *Chou Ritual*, of 12,500 men,” while *fa* means “the left-hand entrance of a triple-gate, as opposed to [*yue*] the right-hand entrance.”³⁷ *Junfa* as “military merit” was used in the Tang. But the compound *junfa* in the sense current today is not found in the classical Chinese vocabulary.³⁸

It was the Japanese who grasped the linguistic possibilities offered by the suffix *fa*, which in Japanese is read *batsu* and means “a clique, a faction, a coterie, a clan.” They used it to create a whole family of terms.³⁹ In addition to *monbatsu* (in Chinese, *menfa*), meaning “pedigree or lineage,” Japanese speak of academic cliques or *gakubatsu* (*xuefa*), regional factions based on the *han* or feudal domains and thus called *hanbatsu* (*fanfa*), and, most familiar to Western readers, financial

³⁵ Mao Zedong, “Why Is It That Red Political Power Can Exist in China?” in *Selected Works of Mao Tse-tung* (Peking, 1967), 1: 63–64. An example of the continued use of the term is Zhang Tongxin, ed., *Guomindang xin junfa hunluan shilue* (Ha-er-bin, 1982). Han Jianfu, “Zhongguo jindai junfazhi yanjiuzhong de jige wenti,” *Guangdong shehui kexue*, 3 (1988): 69–76, reviews much of the historiography.

³⁶ Luo Jialun, ed., *Geming wenxian* (Taipei, 1953-), 14: 564, cited by Ch’en, *Military-Gentry Coalition*, 2.

³⁷ Herbert A. Giles, *A Chinese-English Dictionary*, 2d edn., rev. and enl. (1912; rpt. edn., Taipei, 1972), 399, 412.

³⁸ It is not found in *Ciyuan* (Beijing, 1983). Morohashi Tetsuji, *Dai Kanwa Jiten* (Tokyo, 1955–60), 10: 996, lists one example from the Tang, whose sense is quite different.

³⁹ Koh Masuda, ed., *Kenkyusha’s New Japanese-English Dictionary*, 4th edn. (Tokyo, 1974), 79.

combines, or *zaibatsu* (*caifa*).⁴⁰ *Gunbatsu* follows the same pattern, and for the immediate source of the Chinese *junfa* we must look to Japan.⁴¹

In Japanese, these terms are generally used as collective nouns: thus *gunbatsu* does not mean “a warlord” but rather “the military clique,” denoting “army and navy leaders as an independent political grouping.”⁴² The earliest Chinese usage of the term is similar. In the late 1910s, Chen Duxiu regularly employed not only the term *junfa* but also *caifa*, which was often paired with it in Japanese but which never really caught on in Chinese. And instead of treating *junfa* as plain singular, early writers such as Zhang Junmai (a graduate of Waseda University in Tokyo) used it in its collective, Japanese sense.⁴³

But to trace the Chinese term back to Japanese is of only limited use in understanding the origin of the idea of the warlord. For the Japanese *gunbatsu* is itself a new word, not used in the Meiji period or earlier but only a few years before it entered China, in the Taishō era (1912–1926).⁴⁴ During this time, when the Japanese emperor was weak and the army and navy grew strong in the face of increasing security threats, the military cliques (whose origins lay partially in residual regional loyalties of the *hanbatsu*) began to grow in influence.⁴⁵ Some Japanese saw parallels between their own situation and that of Prussia, and they began to import, for analytical purposes, the concept of “militarism” then being developed in Europe.

The word “militarist” had long served in the West as the focus of a discussion on the origins of war and, specifically, on the relationship between the military and the civil, or between force and society.⁴⁶ As the debate developed, it gradually divided into two schools. One argued that the military and its technology were the source of militarism and believed it could be avoided by democratizing the military by means of militia organization. This was the view of Jean-Jacques Rousseau. Such views fed into subsequent argument over standing armies, which Immanuel Kant and Johann Fichte criticized as “threats to peace and economic prosperity.”⁴⁷ By the late nineteenth and early twentieth centuries, this school had developed the theory that militarism was, in the words of Herbert Spencer, ultimately “a process of regimentation . . . primarily taking place in the army, [which] secondarily affects the whole community.”⁴⁸ A generation later, Norman Angell, building on this approach, contrasted the non-military industrialism “which has . . . given us Canada and the United States” with “stupid and sordid

⁴⁰ *Kenkyusha*, 1125, 310, 393, 2037.

⁴¹ *Kenkyusha*, 354. For additional information on *gunbatsu*, see Janet Hunter, comp., *Concise Dictionary of Modern Japanese History* (Berkeley, Calif., 1984), 129, 218; *Nihon rekishi daijiten* (Tokyo, 1956–60), 4: 181. In “The Ideas and Ideals of a Warlord: Ch'en Chiung-ming (1873–1933),” *Papers on China*, 16 (1962): 198, Winston Hsieh stated that the Chinese term *junfa* is borrowed from Japanese but supplied no source. Although *junfa* is not listed in the standard dictionary of borrowed words in modern Chinese (Gao Mingkai and Liu Zhengtan, eds., *Xiandai Hanyu Wailaizi Yanjiu* [Beijing, 1958]), nevertheless, as is shown below, Hsieh was certainly correct.

⁴² Hunter, *Concise Dictionary of Modern Japanese History*, 129.

⁴³ Zhang, *Guonei zhanzheng liujiang*, 93.

⁴⁴ Yoshio Matsushita, *Meiji no guntai* (Tokyo, 1963), 183–84.

⁴⁵ Yoshio, *Meiji no guntai*, 183–84.

⁴⁶ For an excellent review, see Volker R. Berghahn, *Militarism: The History of an International Debate 1861–1979* (New York, 1982).

⁴⁷ Berghahn, *Militarism*, 8–9.

⁴⁸ Quoted in Berghahn, *Militarism*, 11.

gold-braid" militarism which has "given us Venezuela and Santo Domingo." A related approach, one that opposes militarism not to pacifism but rather to "civilianism" is found in the work of Alfred Vagts.⁴⁹ But such distinctions between militarism and civilianism, or militarist and non-militarist industrialism, would never have been accepted by Marxist thinkers in the second stream. They rejected the idea, implicit in the first approach, that violence had autonomy as a causal force and looked instead for an explanation that found the origin of militarism in the economic system of a society. Their views gained adherents particularly after World War I.

The autonomy of violence was debated in the famous polemic between the German philosopher Karl Eugen Dühring and Friedrich Engels, who attacked him in the *Anti-Dühring* (1894). Dühring had argued that force was an independent factor creating oppression: without a sword, as he put it, Robinson Crusoe could never have enslaved his man Friday. Engels' argument that even slavery was purely economic in its origins proved unsatisfactory, and the problems of "the idealism of violence" and the nature of "militarism" have haunted Marxist thought ever since. V. I. Lenin added little to the debate: the term "militarism" occurs in only fifty-nine passages in his collected works, while Leon Trotsky limited himself to comments on "Bonapartism."⁵⁰ It fell to a younger generation of Marxists to pursue the issue, and by the 1920s they had made substantial progress.

Karl Liebknecht undertook to make a comprehensive socioeconomic analysis that would "lay bare the deepest, most hidden roots of capitalism" by looking at "militarism"; Rosa Luxemburg, who as early as 1899 had called "militarism" "the strongest pillar of [capitalist] class rule," improved on Liebknecht's effort with *The Accumulation of Capital* (1913), which introduced the idea of arms races as important to capitalist development. Further refinements followed. The tendency of war to break out despite its destruction of many of the capitalists who were argued to be fostering it was explained by means of Rudolf Hilferding's concept of the role of "finance capital." What emerged was the argument that the various economic stages of development produced their characteristic military formations and that, as Nikolai Bukharin argued, "the rule of finance capital implies both imperialism and militarism."⁵¹

Such analyses exerted a powerful appeal for certain Japanese trying to understand the politics of their own society. The word "militarism," translated as

⁴⁹ Norman Angell, *The Great Illusion: A Study of the Relation of Military Power in Nations to Their Economic and Social Advantage*, 3d edn. (London, 1911), 187, quoted in Berghahn, *Militarism*, 21 n. 36; Alfred Vagts, *A History of Militarism* (London, 1938), 15, quoted in Berghahn, 9 n. 11.

⁵⁰ Institut Marksizma-Leninizma pri Ts.K. K.P.S.S., comp., *Spravochnyi tom k polnomu sobraniu sochinenii V. I. Lenina*, Part 1 (Moscow, 1978), 384; Leon Trotsky, *The History of the Russian Revolution*, Max Eastman, trans., Vol. 2: *The Attempted Counter-Revolution* (New York, 1932), 136. See also Bernard Semmel, ed., *Marxism and the Science of War* (New York, 1981).

⁵¹ K. Liebknecht, "Militarismus und Antimilitarismus unter besonderer Berücksichtigung der internationalen Jugendbewegung," rpt. in Volker R. Berghahn, ed., *Militarismus* (Cologne, 1975), quoted in Berghahn, *Militarism*, 22; R. Luxemburg, *Gesammelte Werke* (Berlin, 1970), 1.1: 446–66; see also *Die Akkumulation des Kapitals* (Berlin, 1913), quoted in Berghahn, *Militarism*, 24–25; R. Hilferding, *Finance Capital* (Vienna, 1910); and N. I. Bukharin, *Imperialism and World Economy* (1917; rpt. edn., New York, 1966), 127, quoted in Berghahn, *Militarism*, 26. See also Martin Shaw, ed., *War, State and Society* (New York, 1984). How seriously the Chinese take such questions is indicated by the recent publication of a history of the development of the concept of imperialism in the West; Cai Zhongxing, *Diguo zhuyi lilun fazhan shi* (Shanghai, 1987).

gunkokushugi, is found in Japanese sources by 1911.⁵² It quickly took root in the intellectual soil of Japan, giving rise to the strong, indigenous school of theorists including Eitarō Noro, Moritarō Yamada, and Hisao Ōtsuka.⁵³ And it was from such intellectual milieus in Japan that the concepts of militarism and warlordism, along with Marxist concepts more broadly, first entered China.⁵⁴

As mentioned earlier, perhaps the first use of the term *junfa* in modern Chinese was by Chen Duxiu. Chen was an influential figure in the intellectual life of China during this period. A prolific essayist, and among the first Chinese to study and to popularize Marxist ideas, he served as dean of the College of Letters of Peking University from 1917 to 1919, editor of the progressive journal *Xinqingnian*, and in 1920–1921 was instrumental in creating the Chinese Communist party, of which he was the first chairman. Beginning in the 1910s, he took an interest in questions of the military and society. During World War I, Chen had watched Germany closely: the string of German victories in late 1915 led him to hope that that country, “a newly developed nation, could provide leadership to the colonial peoples of the world.”⁵⁵ But Germany disappointed him, and he increasingly adopted the ideas of the German opposition, which he expressed through the vocabulary developed in Japanese for the same purpose. His essays of the time frequently speak of “militarism” (*junguozhuyi*, a recognized loan-word from Japanese).⁵⁶

Chen appears to use the word *junfa* first in 1918, as part of a fascinating and revealing essay written in connection with the defeat of Germany in the European war. When news of the allied victory reached China, celebrations began in many cities. In Tianjin, for example, joyful students constructed a float called a “national spirit boat” [*guohunzhou*].⁵⁷ Two students, one made up as Guan Yu (d. A.D. 219), the general who came in the Qing dynasty to be venerated as the Chinese god of war, and the other as Yue Fei, the patriotic hero of the Song, sat in this boat as it was carried through the streets of the city to the cheers of the populace. This was a celebration that drew on a traditional Chinese vocabulary of martiality: like patriots in Europe who expressed their understanding of the Great War by referring to the age of chivalry and knighthood, the Chinese students of Tianjin looked to the past. Evidently, they saw nothing inherently wrong with warfare or the military per se; they were only concerned that their cause and their nation should be victorious.

Chen found the activities of the Tianjin students deeply discouraging, and he contrasted them with what their Japanese counterparts had done. In Tokyo, five

⁵² Tadao Kabashima, *Meiji Taishō shingō zokugo jiten* (Tokyo, 1984), 109.

⁵³ On this vast topic, see Germaine A. Hoston, *Marxism and the Crisis of Development in Prewar Japan* (Princeton, N.J., 1986).

⁵⁴ On the introduction of Marxism into China, see Arif Dirlik, *Revolution and History: The Origins of Marxist Historiography in China, 1919–1937* (Berkeley, Calif., 1978), esp. 21–53.

⁵⁵ Lee Feigon, *Chen Duxiu: Founder of the Chinese Communist Party* (Princeton, N.J., 1983), 108–09.

⁵⁶ *Chen Duxiu wenzhang xuanbian*, 1: 129, 195, and *passim*; Gao and Liu, *Xiandai Hanyu wailaici yanjiu*, 90.

⁵⁷ The idea of *guohun* or “national soul” was an important feature of new Chinese patriotism, which was developed under the Qing in the late nineteenth and early twentieth centuries. See Laurence A. Schneider, “National Essence and the New Intelligentsia,” in *The Limits of Change: Essays on Conservative Alternatives in Republican China*, Charlotte Furth, ed. (Cambridge, Mass., 1976), 59.

thousand students from Keiō University had marched through the streets to celebrate the victory. But they had borne no images of past Japanese heroes. Rather, they carried lanterns and a huge banner on which were written the three characters *tō gunbatsu* (Down with Warlords).

On the surface, the “warlords” that the Japanese students were criticizing were those of Germany, just defeated. But, as Chen and everyone else understood, the real targets were not German but Japanese “militarists.” In the Taishō period, the influence of military figures in Japanese politics had come under increasing attack. Chen Duxiu mentioned two in particular, Nogi Maresuke (1849–1912), who had commanded the imperial guard at the siege of Port Arthur, and Tōgō Heichahirō, the victor of Tsushima.⁵⁸ Chen could not help but be disheartened by the contrast between the two groups of students: the Chinese were still “worshipping” Guan Yu and Yue Fei, “the Chinese equivalents of Nogi and Tōgō” while the Japanese, by contrast, understood that they were all “militarists” and therefore enemies of the people.⁵⁹

Chen’s presentation of these two vignettes and his interpretation of them strongly suggest that the *junfa* vocabulary was first developed in Japan and then imported into China. But, more important, Chen’s analysis of the Japanese victory celebrations and his comparison of them with the Chinese underscore the fact that the idea of militarism and of the warlord, as Chen wanted them to be understood, were in no sense indigenous to China. They might be powerful tools for understanding Chinese society, but they were not among the categories Chinese traditionally employed for that purpose. To Chen, the new vocabulary was part of a new and different way of thinking about war and violence.

THIS NEW ANALYTICAL APPROACH gained strength in China during the early 1920s. The Japanese-derived *junfa* vocabulary entered the mainstream more and more. European representations of militarists were published in China: a striking example is a cartoon from the German satirical journal *Kladderadatsch* (1848–1944) depicting two Chinese generals as puppets of foreign powers (Figure 4). This image was reproduced in 1925 in the highly influential *Dongfang zazhi* (Eastern Miscellany [1904–1948]).⁶⁰ Also important were influences from the Soviet Union. The Russian civil war in particular provided many parallels to the Chinese internal conflicts of the 1920s. Russia had been as divided as “warlord” China (at one point in 1918, nineteen different self-proclaimed governments had been active), yet the Bolsheviks had ultimately imposed unity.⁶¹

Quite naturally, then, the model for the anti-“warlord” propaganda work of the Guomindang army during the Northern Expedition came partly from the USSR.

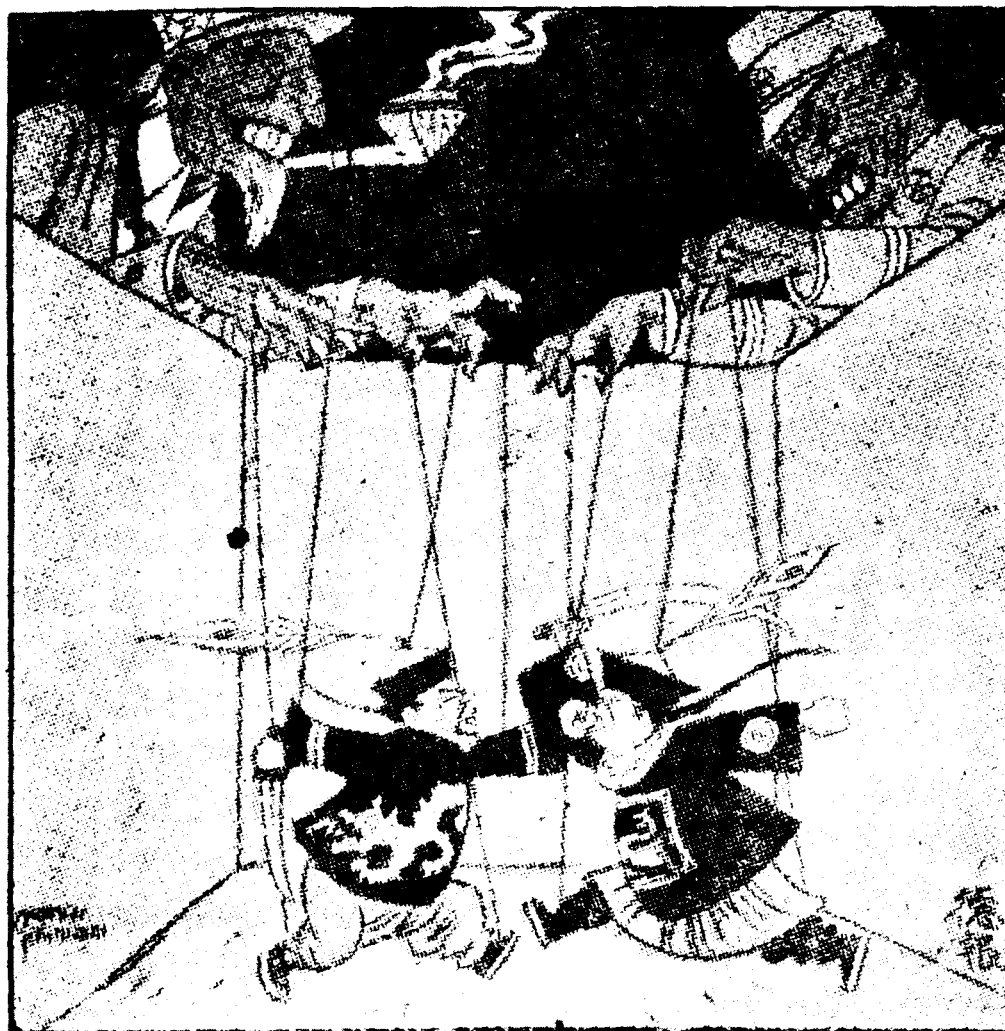
⁵⁸ Nogi, called the “last samurai,” was subsequently military councillor and, from 1907, head of the Peers’ School. Tōgō served as chief of the naval general staff (1905–1909), supreme military councillor, admiral of the fleet (1913), and supervisor of the crown prince’s studies. See the entries in Hunter, *Concise Dictionary of Modern Japanese History*, 150, 227.

⁵⁹ Chen Duxiu, “*Dao junfa*” (December 29, 1918), in *Chen Duxiu wenzhang xuanbian*, 1: 312; originally published in *Meizhou pinglun*, no. 2.

⁶⁰ *Dongfang zazhi*, 22 (1925): 13.

⁶¹ Adam B. Ulam, *A History of Soviet Russia* (New York, 1976), 34.

(一) 畫漫事時際國



柏林“Kladderadatsch”報謂我國內戰表面上
軍閥爲權利而弄兵，內幕却完全受日美兩國的操縱。

FIGURE 4: Cartoon from *Kladderadatsch* reproduced in *Dongfang zazhi* (Eastern Miscellany), 32 (1925): 13.

The Bolsheviks portrayed the White generals just like Chinese “warlords”: militarists supported by prosperous peasants, capitalists, and foreign powers. Aleksandr Kolchak, who had proclaimed a Siberian government in 1918, was described in the first sentences of the Karakhan Declaration of July 25, 1919, as a “counterrevolutionary tyrant who depends upon military might and foreign capital for the strengthening of his own position in Russia.”⁶² V. D. Vilenskii-Sibiriakov, an early Comintern representative who arrived in China in 1919, had

⁶² Wang Jianmin, *Zhongguo Gongchandang Shigao* (Taipei, n.d.), 1: 21, translated in Li, *Modern China*, 187.

worked against Kolchak and the allied intervention in Siberia, and he understandably saw the Chinese situation as parallel.⁶³

Chinese visual images of “warlords” also owe much to Soviet depictions of the Civil War period. Deni (Viktor Nikolaievich Denisov, 1893–1946) portrayed Kolchak, with flag and sword, receiving the tribute of the kulaks and the bourgeoisie (1919).⁶⁴ D. S. Moor (Dmitrii Strakhevich Orlov, 1883–1946) captured the idea that the “militarist” had no power of his own in “Chortova kukla” or “devil puppet” (1920) in which the terrifying figure of the anti-Bolshevik General P. N. Wrangel in the first frame is revealed in the second to be no more than a mask on a stick with a uniform draped over it, all held up by figures representing the Entente (Figure 5).⁶⁵ The clearest immediate model for China, however, is Boris Efimov (1900–), long a cartoonist for official Soviet publications, whose style was widely imitated. Efimov’s visual attacks on the French occupation of the Ruhr and on the Romanian annexation of Bessarabia may have helped create a representational vocabulary for Chinese irredentism, while his sketch of Marshal Józef Piłsudski (1867–1935) sharpening his teeth for the USSR presents the prototypical warlord (Figure 6).⁶⁶

Indeed, shifts in the visual depiction of military themes parallel the terminological transition from *dujun* to *junfa* already mentioned. Early Chinese cartoonists seem to have been influenced by the relatively light-hearted humorous broadsheets of the nineteenth century, such as *Le Charivari* and *Puck* (founded in 1876) in New York City. Shen Bochen (1889–1920), for example, whose *Shanghai Puck* had a circulation of more than 10,000 in Shanghai and the Yangzi Valley, was clearly influenced by such Western models.⁶⁷ His “Struggle between South and North” (“Nanbei zhi zheng”) published in 1918, for example, shows two soldiers, in Western uniform, confronting each other with sword and bayonet, while standing on a prostrate figure who is in traditional garb, labeled “China” (Figure 7). But while the cartoon makes a political statement, it still has a certain whimsy: it remains a cartoon. *Shanghai Puck* ceased in 1918, Shen died two years later, and Chinese political cartooning declined.

When cartooning revived, it had changed. Although Shen Bochen held strong political views, both the original New York *Puck* and his own unauthorized Shanghai imitation were intended, above all, to be funny. By the mid-1920s, however, much Chinese cartooning had become explicitly a political tool of the left. Thus in 1925, a revolutionary broadsheet (*huabao*) portrayed General Wu Peifu (1874–1939) as a dog, led on a leash by Uncle Sam. Lest anyone miss the point, this was labeled “The truth about imperialism and warlords.”⁶⁸ In 1926, the

⁶³ V. N. Nikiforov, *Sovetskie istoriki o problemakh Kitaia* (Moscow, 1970), 67–68.

⁶⁴ *Sovetskii politicheskii plakat iz kolleksii gosudarstvennoi biblioteki imeni Lenina* (Moscow, 1984), 5; Stephen White, *The Bolshevik Poster* (New Haven, Conn., 1988), 58.

⁶⁵ White, *Bolshevik Poster*, 102.

⁶⁶ Bor: *Efimov Karikatury*, L. D. Trotskii, intro. (Moscow, 1924 [Preface, July 20, 1924]), Ruhr, p. 2, Bessarabia, p. 21; Piłsudskii is in L. L. Varshavskii, *Nasha politicheskaiia karikatura* (Moscow, 1930), 57. In 1920, Piłsudskii had carried out a rapid advance across the Pripiet marshes, checked only at Kiev. When the Soviets counterattacked, he stopped them in the so-called Miracle of the Vistula, and the Bolsheviks were forced to yield their Polish conquests.

⁶⁷ See Bi Keguan and Huang Yuanlin, *Zhongguo manhuashi* (Beijing, 1986), 47–56.

⁶⁸ Bi and Huang, *Zhongguo manhuashi*, plate 80.

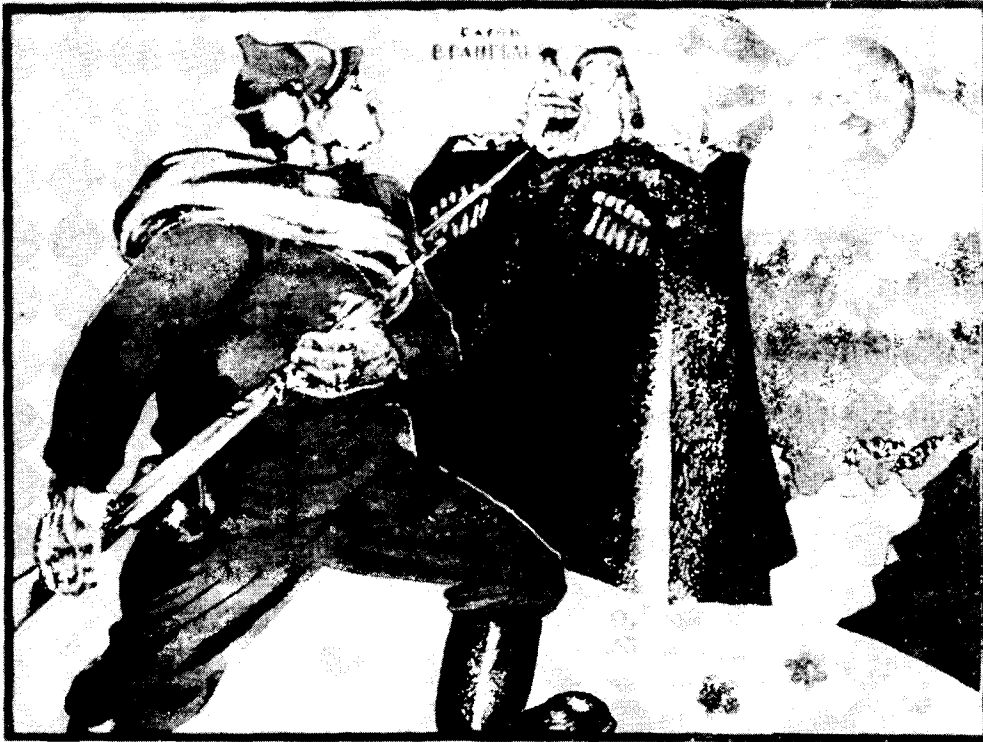


FIGURE 5: D. S. Moor (Dmitrii Strakhevich Orlov, 1883–1946), "Chortova kukla" (the devil puppet), 1920. In Georg Piltz, *Russland wird rot: Satirische Plakate 1918–1922* (Berlin, 1970), plate 66.



FIGURE 6: Boris Efimov (1900–), “Piłsudskii tochtit zuby na SSSR” (Piłsudski sharpens his teeth for the USSR), 1926. In L. Varshavskii, *Nasha politicheskaya karikatura* (Moscow, 1930), 57.

political department of the First Corps of the National Revolutionary Army produced a dramatic sketch of warlordism and imperialism being clubbed down by the leader of angry citizens (Figure 8).

An important artist of this revolutionary period was Huang Zhuohua, who served Peng Pai (1896–1929) in the Peasant Movement Training Institute in Guangzhou.⁶⁹ In August 1926, a striking trio of his drawings appeared in *The*

⁶⁹ Bi and Huang, *Zhongguo manhuashi*, 67–68.

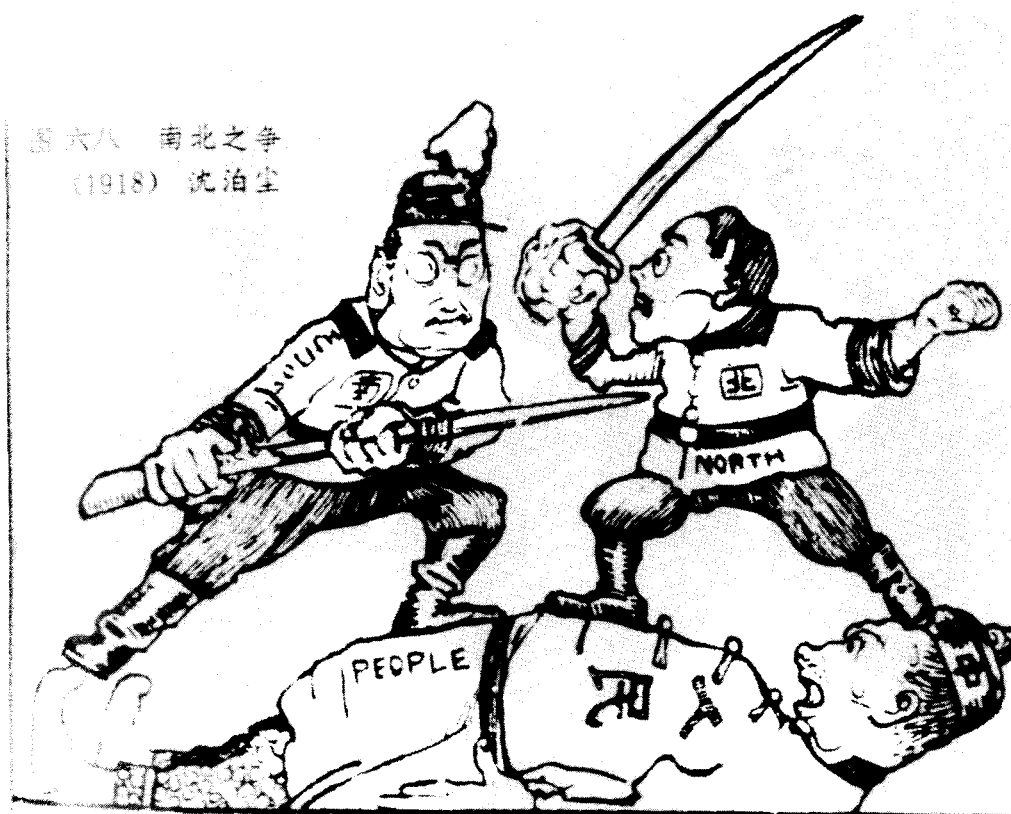


FIGURE 7: "Struggle between North and South," 1918. Characteristic work of Shen Bochen (1889–1920)—satirical and lighthearted but with an unmistakable political message. In Bi Keguan and Huang Yuanlin, *Zhongguo manhuashi* (Beijing, 1986), plate 68.

Plough (*Litou zhoubao*) titled "our past," "our present," and "our future." They showed in succession a mass of people being crushed under a huge boulder on which sat figures labeled "imperialism," "warlords," "compradores," "local bullies," etc.; the same scene with the people beginning to lift the boulder away and topple their oppressors, and finally, the people triumphant with the former oppressors crushed to the ground.⁷⁰

In 1927, as Chiang Kai-shek turned against his former allies, the Guomindang left and the Communists, he too was labeled a "warlord"—one of the "New Warlords of the Guomindang [*Guomindang xin junfa*]." History had prepared the doom of such traitors, however, and a cartoon of 1927 portrayed a toothless, blindfolded Chiang feeling his own way toward his grave, where a bird sat perched whistling "welcome" (Figure 9).⁷¹

⁷⁰ Bi and Huang, *Zhongguo manhuashi*, 67–68, plate 94.

⁷¹ Bi and Huang, *Zhongguo manhuashi*, plate 97. Strikingly similar in composition to A. A. Radakov's blindfolded figure walking off a cliff, used in a Soviet literacy campaign (1920). See *Sovetskii politicheskii plakat*, 29.



FIGURE 8: Soviet-influenced cartoon, prepared by Guomindang First Army political department, 1926. In Bi and Huang, *Zhongguo manhuashi*, plate 95.



图 九七 新军阀蒋介石甘向坟墓里摸索前行 (1927)

FIGURE 9: Chiang Kai-shek approaches his doom, 1927. In Bi and Huang, *Zhongguo manhuashi*, plate 97.

But perhaps the most striking portrayals of war and warlords by this revolutionary generation of cartoon artists in China came from the pen of Huang Wenneng (d. 1934), who drew for the popular and long-established *Dongfang zazhi* and, during the Northern Expedition, worked for the revolutionary forces in police and naval propaganda departments. Huang designed the cover for the special edition of the *Dongfang zazhi* devoted to the incident of May 30, 1925, and his anti-British and anti-American cartooning led to a lawsuit against the magazine.⁷² Huang stressed the putative connection between external imperialism and internal warlordism: one cartoon in 1927 depicted four emaciated figures bearing the place-names of the celebrated incidents of the growing unrest—Hankou, Shaji, Nanjing, Wanxian—labeled (in English) “Made in England” under a sign proclaiming the English-brand imperialism shop.⁷³

⁷² Bi and Huang, *Zhongguo manhuashi*, 76–78.

⁷³ Bi and Huang, *Zhongguo manhuashi*, 108, plate 62.

ALTHOUGH THE CHINESE ADOPTED images, organizations, and rhetoric from Europe and the Soviet Union during the 1920s, they never really adopted the essence of the Marxist understanding of militarism, which saw it as the product of capitalism and imperialism and which thus rejected any suggestion that a militarist held genuine autonomous power. No matter how carefully they couched their arguments in Marxist terms, the Chinese continued to understand that power came from the barrel of a gun and to act accordingly. No matter how hard they tried to be Marxist economic determinists, Chinese on the left ended up as idealists of violence.

No one, not even Chen Duxiu, discarded completely the idea that the *junfa* had some independent power derived from arms and not economics. The problem for Chinese thinkers was not the intellectual status of violence but rather how to rid society of the pathology created when it was unleashed. Some argued that violence should be used against violence: Gao Yihan, as we have seen, was looking for the non-warlord general who would sweep the others away. Chen Duxiu envisioned a powerful party that would crush the warlords. This approach usually included a strong central force reforming China and pulling it together. Still others advocated a federal system of local government as the only solution. The Western argument about militarism, in other words, was transformed in its journey to China. This transformation may be seen clearly in Hu Shi's critiques of the usage of the new (and he thought inappropriate) word *junfa* by Chen Duxiu and Liang Shuming.

In 1922, Chen had elaborated his understanding of warlordism and its origins in an essay, "My Views on the Present Questions of Chinese Politics," which excited considerable controversy. Although Chen devoted plenty of ink to questions of economics and class, his ultimate analysis of violence would nevertheless have horrified Engels. Chen argued that "feudal countries are based on the power of *junfa*." A good Marxist would have known enough to say "based on feudal means of production." Likewise, Chen found the cure not in economic change or class struggle but in the creation of a strong and centralized party that could defeat the warlords and forge a strong and unified state. In this, he argued directly against the then-popular idea that the solution to China's problems would come from decentralization and provincial autonomy within a federal framework. That would only worsen things: for Chen, "the divisive activity of the militarists [was] the sole source of chaos in China."⁷⁴

Hu Shi could not accept this analysis and replied to Chen on September 8, 1922.⁷⁵ He began with Chen's assertion that warlords were "the source of China's political disputes." Hu disagreed: Chen's *junfa* were "no more than a manifestation of the disorder, and by no means its origin." Where, Hu asked, did the "big and small *junfa*" come from? Chen had argued they were "left over from the imperial system." Hu compared this to saying that "rice originates in rice jars" and proposed instead an explanation of the *junfa* that stressed thought and politics and was clearly derived from Chinese tradition. The true source of warlordism,

⁷⁴ Chen Duxiu, "Duiyu xianzai Zhongguo zhengzhi wenti de wo jian," in *Hu Shi Zuopin Ji*, 9 (Taipei, 1986), 82–87, originally published in *Hu Shi Wencun* (Taipei, 1953), 2.3.

⁷⁵ Hu Shi, "Liansheng zizhi yu junfa geju—da Chen Duxiu," in *Hu Shi Zuopin Ji*, 9: 75–82.

according to Hu, was the attempt to unify China by force from above, rather than to build strong local institutions, which could alone bring about lasting unity. According to Hu, both would-be warlord unifiers and those of their enemies who wanted to create a strong party army against them suffered from the same delusion: the belief, which went back to Qin Shihuang, that force could bring unity.

But Chinese history demonstrated that, no matter how strong order imposed from above was initially, it eventually produced division or even disintegration. Real unity came only when the government relied on well-run, self-administering localities gradually to knit themselves and the country together. Such were the lessons of the rebellions of the late Qing. The forces that put down those disorders were not central army divisions but new armies that originated in local defense, such as the Xiang army. The new unity then achieved was the result of the self-defense of the various provinces. This pattern had continued into the twentieth century. For the last sixty years, the authority of the center had daily been shrinking, while local awareness had daily been increasing, and after 1911–1912 provincial autonomy had become a fact. Warlordism originated when, according to Hu, instead of understanding that the action of strong local institutions would eventually create a strong union, leaders attempted once again to use force to create unity. Hu concluded with a characteristically brilliant paradox: it was precisely such an attempt to impose unity militarily that created the divisions of warlordism. And, since attempting military unification had itself been the cause of *junfa* and division, a federal system of provincial autonomy should be a powerful weapon in today's struggle to remove the *junfa*.

Hu extended such analyses in an exchange with Liang Shuming in 1930. In his article "Which Road Are We Going?" written on April 13, 1930, Hu had listed "five enemies" of China, with "disorder" as the fifth. Conspicuously, he did not talk about warlordism, imperialism, or capitalism, although he did mention the dangers posed by self-proclaimed revolution and unbridled violence.⁷⁶ For this, Liang Shuming (1893–1988) took him to task, maintaining in the second issue of his journal *Cunzhi* that "warlordism" was the true source of China's ills. Hu, in his response of July 1930, continued to insist that the origin of warlordism lay in the failure of civil society, not in imperialism. Poverty and internal strife in China long antedated imperialism. The real problem was disorder: "Warlords are a product of disorder."⁷⁷

Hu Shih did not agree with either side in the Western argument about militarism. He did not blame soldiers for it, nor did he blame capitalism and imperialism. Finding the new term *junfa* inappropriate, he propounded a genuinely "Chinese" approach—one that took civil order as both primary and autonomous. Like Zhang Junmai, he saw military authority as fundamentally weak when compared to the civilian and, furthermore, blamed the lack of strong civil government in China not on ambitious soldiers but on degraded intellectuals. Traditionally the moral arbiters of society, China's intellectuals had become

⁷⁶ In *Hu Shi Wencun*, 4th ser. (Taipei, 1953), 429–44; also translated in abbreviated form in Hu Shih and Lin Yu-t'ang, eds., *China's Own Critics* (1931; rpt. edn., New York, 1969), 11–21.

⁷⁷ Liang Shuming, "Jing yi qingjiao Hushizhi xiansheng," in *Cunzhi*, 1 (1930): 1–8; also in Liang, *Zhongguo minzu ziji yundong zhi zuihou juewu* (Shanghai, 1936), 381–91.

dependent and opportunistic (*zougouhua*, literally, “running-dog-ified”). Hu then turned the entire argument around:

My Teacher [Liang] says, “disorder is all the result of activities of the warlords,” but this is not in accordance with historical fact. Warlords are the product of disorder, and disorder is by and large created by our friends in long gowns [intellectuals]. Of all the so-called revolutions of the last twenty years, which one was not created by intellectuals [*wenren*]? Of the warlord struggles of the last twenty years, which one was not stimulated by disappointed political opportunists [*zhengke*]? Of the local violence of the Communist party [The year 1927 had seen an unprecedented campaign of terrorism], which case has not been fanned and organized by the comrades in long gowns? These three categories account for 70 or 80 percent of chaos.

Disorder and warlordism, to Hu Shi, were neither the products of the social contradictions of capitalism, as Liebknecht would have argued, nor of the men in gold braid, as Angell would have. Returning to the Chinese understanding that, at least in their polity, *wen* (civil) will dominate *wu* (military), Hu argued that they were ultimately the product of civilian politics.⁷⁸

Something very interesting has happened here. Neither Chen nor Hu argued a Western point of view. Their argument may seem on the surface to be about militarism, a Western concern. But, at a deeper level, it is about two abiding concerns of Chinese political thought: the moral responsibility of the individual and the question of central versus local government. Chen stressed the role of a single individual as a centralizer and was thus, in effect, an advocate of the *junxian* approach to organizing China; Hu drew on the decentralizing ideas represented by the *fengjian* tradition.⁷⁹ The central economic and historical concerns of the Western argument were missing from the Chinese debate.

AFTER CHIANG KAI-SHEK’S at least partially successful unification of China, the environment and the debate shifted yet again, in ways that began to leave the “warlord” behind. True, the 1930s were the most favorable years for Chinese cartoon publications, with nearly twenty being published in the Shanghai area, and they saw the left produce much polemic and many images making use of “warlord” themes and iconography.⁸⁰ But, for the idea of the “warlord” proper, this was a period of transition. With the initiation of the United Front against the Japanese in 1936, many of the “old” warlords became deeply involved in national defense efforts, while Chiang Kai-shek became the unquestioned national leader whom no one, not even the Communists, was willing to denounce as a “new warlord.” Attack shifted to the Japanese “militarists” and their collaborators. Japanese leaders and soldiers were portrayed much as “warlords” had been earlier, as in the work of Zhang E (1910–), a member of the League of Left-Wing Writers.⁸¹

⁷⁸ “Da Liang Shuming xiansheng” (19–7–29, i.e. July 29, 1930), in *Hu Shi Luncun*, 4th ser. (Taipei, 1953), 444–46.

⁷⁹ For these concepts, see Kung-chuan Hsiao, *A History of Chinese Political Thought*, Vol. 1: *From the Beginnings to the Sixth Century A.D.*, F. W. Mote, trans. (Princeton, N.J., 1979).

⁸⁰ See table in Bi and Huang, *Zhongguo manhuashi*, 94.

⁸¹ Bi and Huang, *Zhongguo manhuashi*, 125–30, plates 171, 173.

But the figures of the 1930s were not “warlords” as we have been treating them. Indeed, the term was acquiring a certain archaic, even affectionate, flavor by the late 1920s, when Winston Churchill was sometimes called a “War Lord.”⁸² The concept was inadequate to deal with Fascists and Nazis. In response, the linguistic and visual vocabulary grew. Boris Efimov’s caricatures of Joseph Goebbels and Adolf Hitler transcended the “warlord” genre and pushed into the territory of generalized militaristic social evil, creating in the process a highly influential set of new images.⁸³

In the meantime, students of China, both in Japan and the West, were beginning to rework their understandings of the 1920s to produce what is, in essence, the accepted approach today. Strict sinology, concerned with etymology, translation, and narrative, was on the decline. In its place came the sociological approach, which found the idea of “warlordism” congenial. It became, for the first time, an object of study and scholarly inquiry. Many Japanese used the term, and this eventually spawned a vast literature (although other Japanese retained the earlier vocabulary: Naitō Konan, for example, writing in 1924 about contemporary China, still spoke of *dujun* politics [*tokugun seiji*] and not *junfa*).⁸⁴ In the West, the word “warlord”—now translated back from the Chinese *junfa*—caught on quickly.

Edgar Snow first heard of “warlords” during his initial trip out East in 1928. He recalled how a young Chinese student he met aboard ship had announced “‘China is on the road at last!’ . . . ‘Domination by the warlords is ended,’ he assured me. I had only a vague notion of what a warlord was then but I was ready to assume that it was beneficial for China that they had been scuppered.”⁸⁵ Harley Farnsworth MacNair, lecturing at the University of Chicago in the spring of 1930, named one session “The War Lords, 1916–1928.”⁸⁶ The Westerners, like the Chinese, proved unable to decide whether “warlordism” was something new or a feature of Chinese society since time immemorial. R. H. Tawney drew a parallel between Chinese society and the history of Europe: “To suppose that China is unique in her political disorders is an illusion. They are the characteristic, not of a country, but of a phase of civilisation, from which other societies have painfully emerged, but from which . . . China is only now emerging.”⁸⁷ Fei Xiaotong is one of many Chinese who were probably influenced by such accounts and who used their general frameworks as models for their own theorizing about social structure in China.⁸⁸ But while Tawney and Fei would make “warlordism” a feature of one stage, eventually passed, in social evolution, historian John Fairbank saw it as one of the “repetitive phenomena” of a “distinctive and

⁸² For example, “Winston Churchill—A British War Lord,” *Current History*, January 1929.

⁸³ See N. Vladimirkii, ed., *Za prochnyi mir, protiv podzhigatelei voyny: Risunki B. Efimova* (Moscow, 1950), for many examples.

⁸⁴ Naitō Konan, *Naitō Konan zenshū*, Kanda Kiichiro and Naitō Kenkichi, eds., 14 vols. (Tokyo, 1966–74), 5: 523.

⁸⁵ Edgar Snow, *Far Eastern Front* (New York, 1933), 155.

⁸⁶ Later published in Harley Farnsworth MacNair, *China in Revolution: An Analysis of Politics and Militarism under the Republic* (Chicago, 1931). See also Kurt Bloch, “Warlordism: A Transitory Stage in Chinese Government,” *American Journal of Sociology*, 43 (March 1938): 691–703.

⁸⁷ R. H. Tawney, *Land and Labor in China*, Barrington Moore, Jr., intro. (1932; rpt. edn., White Plains, N.Y., 1966), 167–68.

⁸⁸ See the “Introduction” by Robert Redfield to *China’s Gentry: Essays in Rural-Urban Relations by Hsiao-Tung Fei*, rev. and ed. by Margaret Park Redfield, (Chicago, 1953), 15.

persistent Chinese pattern” characterized by repeated cycles of “dynastic disintegration, warlordism, and re-unification.”⁸⁹

Along with such attempts to deal with “warlordism” as a phenomenon, there emerged among Westerners in the 1930s and 1940s a new popular account of “warlords,” which in its own way was as preposterous as the earlier accounts of *tuchüns*. Americans who spent time in Chongqing seem to have shared a common picture, presented here by Theodore White. “As the old system of government withered away, the war lords assumed complete sway over the lives of the peasants and fought among themselves wars that were as comic and barbarous as any ever recorded. The war lords were colorful figures; they lived joyfully with many concubines in great mansions, waxed fat on the opium trade, extorted taxes from the peasantry fifty years in advance, wrung land from the original owners to add to their own estates. In the process they became great manorial barons, full of wealth and pride.”⁹⁰ “Warlords” had ceased to be modern or even new; they were a colorful if evil archaism.

For hard denunciation, we can look to the new anti-fascist vocabulary, or to anti-imperialism and anti-Americanism. In the early 1940s, Edgar Snow saw Mao Zedong sympathetically as a “Red Warlord,” while he found Chiang Kai-shek repugnantly “nazified.”⁹¹ Just how diminished the old category of “warlord” had become by the end of World War II can be seen in both Soviet and Chinese portrayals of Chiang in the late 1940s and after. No longer a threatening figure, he is a little tailor embroidering a Guomindang star on an American flag or a Lilliputian Chinaman with buckteeth, clinging to the nose of the American secretary of state John Foster Dulles (Figures 10–11).⁹²

With the end of an active civil war in the 1950s, “warlord” ceased to be a living term in Chinese. It was an important word in orthodox Communist historiography and figured regularly in formulaic political denunciations, as in the *People’s Daily* on Peng Dehuai (1898–1974): “[A] totally capitalist great warlord.”⁹³ But it seemed unlikely ever again to become a “keyword” in either politics or scholarly debate. That verdict, however, came into question after the Tiananmen Square massacre of June 1989. Once more, the role of the military became the key question in Chinese politics, and as that happened, the polemics and the analytical questions of the 1920s gradually regained prominence.

This was certainly clear in rhetoric. In June 1989, demonstrators outside Chinese missions abroad cried, “Down with the Yang family warlords!” [“Yangjia junfa!”]⁹⁴ Others shouted, “Down with the fascist new warlords!” In Paris, the

⁸⁹ John King Fairbank, *Trade and Diplomacy on the China Coast: The Opening of the Treaty Ports 1842–1854* (1953, 1964; rpt. edn., Stanford, Calif., 1969), 7. I have rearranged the order of phrases here but have not, I trust, distorted Professor Fairbank’s meaning.

⁹⁰ Theodore H. White and Annalee Jacoby, *Thunder out of China* (New York, 1946), 4.

⁹¹ Edgar Snow, *People on Our Side* (New York, 1944), 207, 209.

⁹² Bi and Huang, *Zhongguo manhuashi*, plate 331; *Satira na vragov mira: Risunki khudozhnika M. Abramova stikhi poeta Sergeia Mikhailova* ([USSR], 1957).

⁹³ August 16, 1967, quoted in Jürgen Domes, *Peng Te-huai: The Man and the Image* (London, 1985), 121.

⁹⁴ Yang Shangkun b. 1907, vice-chairman of the Central Military Commission; Yang Baibing, political commissar of the Beijing military region; and Chi Haotian, political commissar of the Jinan military region, all believed to be relatives by blood or marriage.

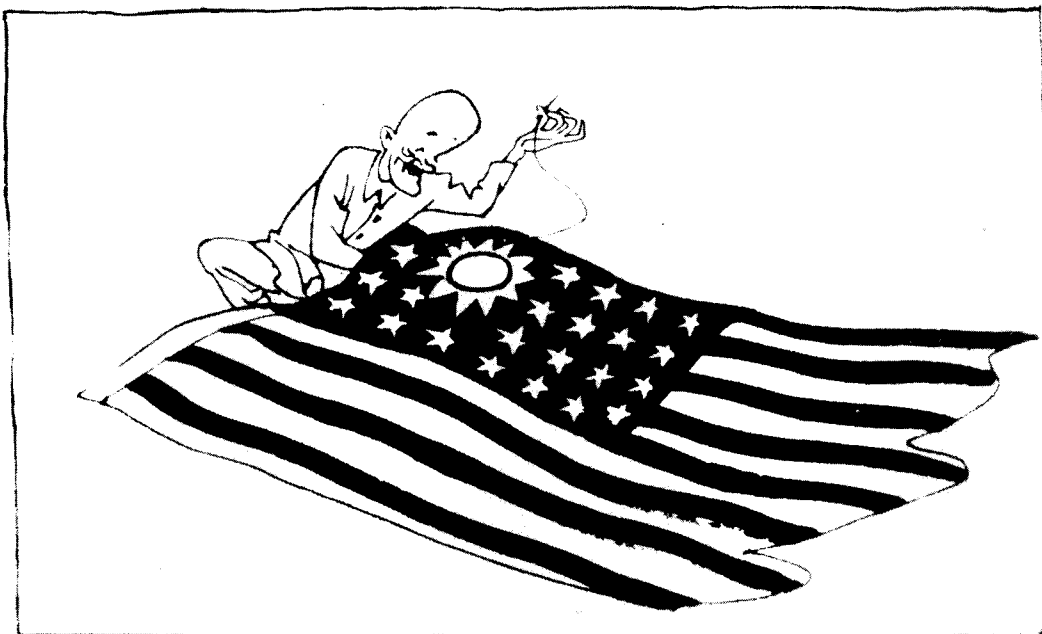


图 三三一 偷天换日 (1947) 特 伟

FIGURE 10: The warlord diminished. Chiang Kai-shek in the late 1940s. Bi and Huang, plate 331.

student leader Wuer Kaixi "called the Communist Government 'a pack of reactionary warlords and fascists' while predicting its demise in six months to three years."⁹⁵ The return to old themes was also increasingly evident in political argument. Questions once asked only about the "warlords" of the 1920s are now being asked about the present as well. Lucian Pye has argued that "warlords established the fact that in modern China political power cannot be divorced from military power." This is certainly true. But it is also true that, as James Sheridan has responded, "the failure of the warlords demonstrated that military power alone is an inadequate basis for political power in China."⁹⁶ If the "fascist new warlords" lack an adequate basis for power, what then will replace them? Some hope, as Gao Yihan and Wu Zhihui did, that military means will somehow solve the problem: a good army or a good general will appear. (Recall the hopes that one or another of the Chinese armies would intervene to stop the repression in June 1989.) But it is striking that others reject violence altogether, and the leading intellectual Yan Jiaqi, now in exile, is calling for "federal solutions" to China's problems, just as Hu Shi and the liberals did in the 1920s.⁹⁷

⁹⁵ Author's personal information; *New York Times* (June 30, 1989): A6.

⁹⁶ Lucian W. Pye, *Warlord Politics: Conflict and Coalition in the Modernization of Republican China* (New York, 1971), 41, quoted in James E. Sheridan, "The Warlord Era: Politics and Militarism under the Peking Government, 1916–28," in *Republican China 1912–1949, Part I*, John K. Fairbank, ed., *The Cambridge History of China*, vol. 12 (Cambridge, 1983), 319.

⁹⁷ Yan Jiaqi, "Zhongguo bing bu shi 'gongheguo,'" *Huaqiao erbao* (New York), (July 24, 1989): 8, and (July 25, 1989): 8, originally published in *Ming bao* (Hong Kong) (July 23 and 24, 1989); see also his remarks, "Weilai Zhongguo sibuqu," in First Congress of Chinese Students and Scholars in the U.S.A., *Conference News* (Chicago), no. 3 (July 29, 1989): 7. For a summary, see Arthur Waldron, "Warlordism versus Federalism: The Revival of a Debate?" *China Quarterly*, 121 (March 1990): 116–28.



FIGURE 11: M. Abramov, "Predstavitel' i ego Pokrovitel'" (Representative and Patron). The cartoon shows Chiang Kai-shek perched on the end of the nose of John Foster Dulles. The caption states: "Giving support to Taiwan puppets USA in every way obstructs the entry into U.N. of 600 million Chinese people." *Satira na vragev mira. Risunki khudozhnika M. Abramova stikhi poeta Sergeia Mikhailova* ([USSR], 1957).

A sixty-year cycle has been completed in China since the end of the Northern Expedition, and political debate in China seems to have returned to where it was then: to basic issues of the nature of civil society, the status of force, and how to break once and for all the patterns of internal political violence that led to the creation of the vocabulary of the *junfa* in the first place.

African Dimensions of the Stono Rebellion

JOHN K. THORNTON

THE STONO REBELLION OF 1739 was one of the largest and costliest in the history of the United States. In studying it, historians have generally not appreciated the extent to which the African background of the participants may have shaped their decision to revolt or their subsequent actions. This essay addresses this upheaval in South Carolina in terms of its African background and attempts to show that understanding the history of the early eighteenth-century kingdom of Kongo can contribute to a fuller view of the slaves' motivations and actions.

In some ways, the failure to consider the African background of the revolt is surprising, since a number of historians have recently explored the possibility of African religions, cultures, and societies playing an important role in other aspects of South Carolina life. Peter Wood, author of the richest examination of Stono, was one of the pioneers in considering African competence at rice growing as important in shaping the decisions of slave buyers, a point followed up in great detail by Daniel Littlefield.¹ Wood also argued that the African origins of the slaves can do much to explain a range of behaviors, from health patterns to language.² Tom Shick showed that African concepts of health and healing influenced the development of folk medicine in South Carolina, and Margaret Washington Creel explained religious development in terms of African religion and religiosity.³

Historians have yet to apply the same sort of approach to the Stono Rebellion. Scholars of the United States interested in the African background of American history have usually sought general information about African culture by reading the accounts of modern anthropologists and ethnologists, which are not always

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¹ Peter B. Wood, *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (New York, 1974), 56–62; Daniel Littlefield, *Rice and Slaves: Ethnicity and the Slave Trade in Colonial South Carolina* (Baton Rouge, La., 1981).

² Wood, *Black Majority*, 63–91, 167–91.

³ Tom W. Shick, "Healing and Race in the South Carolina Low Country," in Paul Lovejoy, ed., *Africans in Bondage: Studies in Slavery and the Slave Trade* (Madison, Wis., 1986), 107–24; Margaret Washington Creel, *A Peculiar People: Slave Religion and Community Culture among the Gullahs* (New York, 1988).

helpful for understanding specific historical situations. Appreciating the African roots of the Stono Rebellion, for example, requires a specific understanding of the kingdom of Kongo between 1680 and 1740 rather than simply a broad understanding of African culture. Historians of Africa, who have access to this type of specific information, have, unfortunately, rarely used it in a way helpful to their colleagues in U.S. history.⁴

Although the Stono Rebellion was very important in the history of South Carolina, it was not well documented. Only one eyewitness account is extant, supplemented by several secondhand reports.⁵ Many English residents of South Carolina, including the anonymous author of the best account, believed that the revolt was somehow precipitated by Spanish propaganda and was part of the larger set of tensions that led to war between England and Spain in 1740. English officials reported a number of Spanish vessels acting suspiciously in English waters, and some Spaniards, including priests, were reported to have made surreptitious visits to South Carolina.⁶ Among other things, these Spaniards were believed to be stirring up the slaves, offering freedom to any who would run away, as indeed many did.⁷

The actual rebellion broke out on Sunday (normally a slaves' day off), September 9, 1739, led by a man named Jemmy and including a core of some twenty "Angolan" slaves. The Spanish were suspected in the uprising because, according to the account, the slaves were Catholics, and "the Jesuits have a mission and school in that Kingdom [Angola] and many Thousands of the Negroes profess the Roman Catholic Religion." In addition to the sentiments of a common religion, many slaves could speak Portuguese, which was "as near Spanish as Scotch is to English,"⁸ thus increasing their receptivity to Spanish offers and propaganda.

The rebels seized a store of firearms and marched off on a trail of destruction and killing, with two drums and banners flying, which attracted a large crowd of slaves. Having reached over sixty in number, they paused at a large field and "set to dancing, Singing and beating Drums to draw more Negroes to them." Shortly afterward, now numbering ninety, they met a force of militia, and a battle resulted. The slaves were dispersed, though not without "acting boldly" and

⁴ The problem is compounded by linguistic difficulties: to do historical studies of eighteenth-century Angola, the most important African source of slaves for South Carolina before Stono, one needs to consult sources written in Portuguese and Italian, very few of which are available in translation and many of which are unpublished or have only been published in recent years.

⁵ See "An Account of the Negroe Insurrection in South Carolina" (undated, ca. 1740), in Allen D. Candler and William J. Northern, eds., *Colonial Records of the State of Georgia* (1904–16; rpt. edn., New York, 1970), 22/2: 232–36. The most useful secondhand accounts are "A Ranger's Report of Travels with General Oglethorpe, 1739–42" (diary), in William D. Merenes, ed., *Travels in the American Colonies* (New York, 1916), 222–23; and William Stephens, *A Journal of the Proceedings in Georgia, Beginning October 24, 1737 . . .*, 2 vols. (London, 1742), 2: 128–30 (this journal was also reprinted in Candler and Northern, *Colonial Records*, vol. 4). This last document is closest in time, dated September 13, 1739, just four days after the revolt.

⁶ Some of these events are reported in Stephens, *Journal*, 2: 77, 78. In retrospect, Stephens was glad that earlier they had stopped a priest on the Georgia coast; 130.

⁷ The earlier incidents are detailed in Wood, *Black Majority*, 309–12; on runaways in Spanish Florida, see Jane Landers, "Gracia Real de Santa Teresa de Mose: A Free Black Town in Spanish Colonial Florida," *AHR*, 95 (February 1990): 9–30.

⁸ Candler and Northern, "Account of Negroe Insurrection," 233.

leaving some dead.⁹ The slaves were not finished, however, for they re-formed and continued toward the Spanish possessions around St. Augustine, one body of about ten fleeing until caught by mounted troops the next day.¹⁰ A week later, another group fought a pitched battle with pursuing militia about thirty miles south of the initial skirmish.¹¹

While the immediate causes of the revolt clearly lay in the difficult conditions of slavery in South Carolina, detailed in Wood's analysis of the colony and the revolt,¹² several elements in the eyewitness account suggest that, along with English mistreatment and Spanish promises, the African background of the slaves contributed to the nature of the revolt. A study of the African background supports the following interpretation: first, South Carolina slaves were in all likelihood not drawn from the Portuguese colony of Angola (as the account implies) but from the kingdom of Kongo (in modern Angola), which was a Christian country and had a fairly extensive system of schools and churches in addition to a high degree of literacy (at least for the upper class) in Portuguese. In its creole form, Portuguese was also a widely used language of trade as well as the second language of educated Kongolese. The Kongolese were proud of their Christian and Catholic heritage, which they believed made them a distinctive people, and thus Kongolese slaves would have seen the Spanish offers in terms of freedom of religion (or rather, freedom of Catholic religion) as additionally attractive beyond promises of freedom in general.

Second, throughout the eighteenth century, Kongo was disturbed by sporadic and sometimes lengthy civil wars, which resulted in the capture and sale of many people, no small number of whom would have been soldiers with military training. Significant changes in the organization and training of armies that were occurring at the same time had increased the number of soldiers trained in the use of firearms, thus increasing the likelihood that such soldiers would be enslaved. These ex-soldiers might contrast with the untrained villagers often netted by slave raids, judicial enslavement, or other means by which slaves ended up being sold to the Americas. Former soldiers might have provided the military core of the rebels, who fought on after their first engagement and generally gave a good account of themselves.

FROM PATTERNS OF THE ENGLISH SLAVE TRADE TO CENTRAL AFRICA, we know it is unlikely that the "Kingdom of Angola" to which the author of the main account of the Stono Rebellion referred was the Portuguese colony known as Angola, largely because the colony sent its slaves on Portuguese and Brazilian ships to Brazil and not to English shipping bound for North America.¹³ Rather, the

⁹ Candler and Northern, "Account of Negroe Insurrection," 234–35; Stephens, *Journal*, 128.

¹⁰ Stephens, *Journal*, 129, 235.

¹¹ Andrew Leslie to Philip Bearcroft, January 7, 1740, quoted in Stephens, *Journal*, 318–19; see also Frank Klingberg, *An Appraisal of the Negro in Colonial South Carolina* (Washington, D.C., 1941), 80.

¹² Wood, *Black Majority*, 271–320.

¹³ For a full discussion of the legal strictures as well as the substantial smuggling, see Joseph C. Miller, *Way of Death: Merchant Capitalism and the Angolan Slave Trade, 1730–1830* (Madison, Wis., 1988), 245–83.

author surely meant the general stretch of central Africa known to English shippers as the Angola coast. This area included coastal parts of modern Zaire, Congo-Brazzaville, and Gabon as well as Angola. The English slave trade, conducted in this period by the Royal African Company, fixed its operations on the northern part of the coast, especially at the town of Kabinda, just north of the mouth of the Zaire River.¹⁴ The importance of Kabinda is underscored by the fact that, according to company records, every central African voyage the company undertook in the 1720s gave Kabinda as its destination.¹⁵ Kabinda was the capital of an independent state, which, lying as it did against a sparsely inhabited interior, did not procure many slaves itself but served as an export station for suppliers coming from the south. Since Kabinda's first line of supply was its southern neighbor across the Zaire, the "Angolans" of the Stono Rebellion most likely came from Kongo. Eighteenth-century visitors such as James Barbot often left from Kabinda to visit other ports along the Kongo coast to the south or up the Zaire River to the town of Nzari (Zaire).¹⁶ Accounts of the 1760s refer to a brisk trade in Kongo's province of Mbula lying on the Zaire and upstream of Nzari, which supplied English, French, and Dutch merchants (probably based at Kabinda) as well.¹⁷

The possibility that people from other parts of central Africa were involved in the rebellion cannot be completely ruled out, however, since Kabinda was also served by the Vili trading network. Centered in the kingdom of Loango, farther north up the coast, Vili traders had built a series of towns since at least the mid-seventeenth century under a disciplined caravan system across Kongo that supplied many of their slaves,¹⁸ but they also contacted suppliers outside Kongo. The Vili engaged in trade with Portuguese Angola despite colonial attempts to prohibit it, and they dealt extensively with Matamba, Angola's independent eastern neighbor.¹⁹ In eastern Kongo, they contacted the merchants of Kongo's marquisate of Nzombo, whose operations extended to the east and included many non-Kongolese.²⁰ Nzombo merchants capitalized on the expansion of the Lunda empire in the second quarter of the eighteenth century to supply slaves to the Vili, although, before 1740, such slaves would not have been available in large numbers.²¹ Likewise, Vili merchants operating to the east and north of Kongo

¹⁴ A general survey of the English trade of this coast is presented in Phyllis Martin, *The External Trade of the Loango Coast, 1576–1870* (Oxford, 1972), 75–130.

¹⁵ David Galenson, *Traders, Planters, and Slaves: Market Behavior in Early English America* (Cambridge, 1986), 164–65.

¹⁶ James Barbot, "Abstract of a Voyage to the Kongo River and Kabinda in 1700," in Thomas Astley, ed., *A New General Collection of Voyages and Travels . . .*, 5 vols. (London, 1746), 3: 202–09.

¹⁷ Cherubino da Savona, "Breve ragguaglio del 'Regno di Congo, e sue Missione' scritto dal Padre Cerubino da Savona, Missionario Apostolico Capuccino" (MS of 1775), fols. 42, 44; mod. edn. in Carlo Toso, ed., "Relazioni inedite di P. Cherubino Cassinis da Savona sul 'Congo e sue Missioni,'" *L'Italia francescana*, 45 (1975): 136–214. A French translation is available, Louis Jadin, "Aperçu de la situation du Congo en 1760 et rite d'élection des rois en 1775, d'après le P. Cherubino da Savona, missionnaire au Congo de 1759 à 1774," *Bulletin, Institute historique belge de Rome*, 35 (1963): 343–419.

¹⁸ For a detailed discussion on trading networks in Angola and its eastern regions, see Miller, *Way of Death*, 207–83.

¹⁹ See Martin, *External Trade*, 130; Miller, *Way of Death*, 200–03.

²⁰ On the Nzombo traders, see da Savona, "Breve ragguaglio," 136–214.

²¹ For an assessment of the Lunda expansion, see John Thornton, "The Chronology and Causes of Lunda Expansion to the West, ca. 1700–1852," *Zambia Journal of History*, 1 (1981): 1–13. Kongo

could buy slaves at the busy markets around the Malebo Pool, which was served by merchants originating higher up the Zaire River.²²

Thus merchants based at Kabinda would buy slaves from an extensive area of central Africa. Nevertheless, the majority enslaved in the period between about 1710 and 1740 were probably from Kongo. First of all, the kingdom was the principal supplier to Kabinda-based merchants and to Vili merchants as well. Second, the eastern regions served by merchants in Nzombo and the Malebo Pool were not as fully engaged in the slave trade in the early eighteenth century as they would become by mid-century, especially after the arrival of Lunda armies in the Kwango region around 1750. Third, the southern regions were jealously guarded by the Portuguese authorities, who sought to block trade northward with a fort at Nkoje built in 1759 specifically to stop Vili traders.²³ Even though the Portuguese could not stop northbound trade completely—it continued to reach Kabinda by overland or coastal routes and French, English, and Dutch merchants based on the north coast—they probably did limit it considerably.

THE BACKGROUND OF TRADE points strongly to a Kongo origin of the Stono slaves, but it is their adherence to Catholicism that confirms it. Only two countries in central Africa were Christian: Kongo and the Portuguese colony of Angola. Missionaries based in Angola did some work in eastern areas outside colonial control, such as Matamba, but they were limited by a shortage of missionaries in the eighteenth century.²⁴ Even in the best of times, these missions were typically of short duration and did not lead to the establishment of a long-lasting, permanent church organization.²⁵ In eastern Angola, moreover, admission of missionaries and acceptance of baptism was usually linked to surrender to

(presumably Nzombo) participation in the trade of Lunda slaves was noted in 1756 by the Portuguese traveler Manuel Correia Leitão, "Relação brève summário da viagem que eo, o sargento-mor dos moradores do Dande fiz as remotas partes de Cassange e Olos . . . 15 de agosto de 1756," in Gastão Sousa Dias, "Uma viagem a Cassange nos meados do século XVIII," *Boletim da Sociedade de Geografia de Lisboa*, 56 (1938): 19–20, 25. Note that Cherubino da Savona had Lunda (Mollua) as one of Kongo's neighbors by 1760; da Savona, "Breve ragguaglio," fol. 41v.

²² A description of the Malebo Pool markets in the late seventeenth and early eighteenth centuries can be found in Luca da Caltanisetta, "Relatione del Viaggio e Missione fatta per me Fra' Luca da Caltanisetta . . . nel 1691 al . . ." (MS of 1701), fols. 55–60v; published in Romain Rainero, ed., *Il Congo agli inizi del settecento nella relazione di P. Luca da Caltanisetta* (Florence, 1974); French translation, François Bontinck, *Diaire congolaise, 1690–1701* (Brussels, 1970). See also Marcellino d'Atri, "Giornate Apostoliche fatte da me Fra Marcellino d'Atri, Predicator Cappuccino nelle missioni de Regni d'Angola e Congo . . . 1690" (MS of 1708), fols. 306–34; mod. edn., Carlo Toso, *L'anarchia Congolese nel Sec. XVII: La relazione inedita di Marcellino d'Atri* (Genoa, 1984). For the history of the commercial group upriver, see Robert Harms, *River of Wealth, River of Sorrow: The Central Zaire Basin in the Era of the Slave and Ivory Trade* (New Haven, Conn., 1981).

²³ Miller, *Way of Death*, 277–78, 582–85.

²⁴ See, for example, Anselmo da Castelvetro, "Relatione dello stato in cui presentemente si trovano le Missioni Cappuccini in Regno di Congo," October 14, 1742, Scritture riferite nel Congregazioni Generali, vol. 712, fols. 296–305. Archivio "De Propaganda Fide" (Rome). Also, *ibid.*, Acta, vol. 112 (1742), fol. 422. On the general state of the Capuchin mission, and other ecclesiastical bodies in Kongo and Angola, see Graziano Saccardo [da Leguzzano], *Congo e Angola, con la storia del missione dei Cappuccini*, 3 vols. (Venice, 1982–84), vol. 2, *passim*.

²⁵ An extreme example comes from Kasanje in the 1660s: see Giovanni Antonio Cavazzi, "Missione Evangelica al Regno di Congo" (MS of 1665), Book 3, MSS Araldi Family (Modena).

Portugal, which slowed missionary work and restricted mass conversions.²⁶ The slaves from the east of Kongo, supplied by Nzombo or Vili merchants, were definitely not Christians according to the Italian Capuchin missionary in Kongo, Cherubino da Savona, who referred to them as “heathens.”²⁷

Of these two Christian countries, Angola was unlikely to export slaves to English ships, especially Christian slaves who would surely have been drawn from areas under direct Portuguese control, where Portuguese restrictions were strongest. That Christian slaves in English hands would be from Kongo is substantiated by complaints of the Spanish Governor Antonio de Salas of New Granada (Colombia) to the Spanish crown in 1735. He stated that English merchants representing the South Sea Company (drawing on the same sources as the English supplying South Carolina) were introducing “black Christians of the Congo” into Cartagena.²⁸

These same “black Christians of the Congo” were the leading rebels of the Stono Rebellion, susceptible to Spanish and Catholic propaganda. They came from a culture well disposed toward Catholics. The Kongolese of the eighteenth century regarded their Christianity as a fundamental part of their national identity,²⁹ since the kingdom was voluntarily converted with the baptism of King Nzinga Nkuwu as João I in 1491, not linked to submission to Portugal as many of the eastern Angolan conversions were. It had independent relations with Rome and its own internally developed church and school system.³⁰

This locally rooted Christian tradition is sometimes regarded as less than orthodox. Most modern scholars have pointed out that the Kongolese simply added Christian labels to their indigenous beliefs. Foreign clergy who worked in Kongo in the seventeenth and eighteenth centuries often regarded them as “Christians in name only,” although some other accounts praise them as model Christians.³¹ But, whatever modern scholars or some eighteenth-century priests thought, the Kongolese regarded themselves as Christians. The elite carefully maintained chapels and sent their children to schools, and the ordinary people learned their prayers and hymns, even in the eighteenth century, when ordained

²⁶ Beatrix Heintze, “Luso-African Feudalism in Angola? The Vassal Treaties of the Sixteenth to Eighteenth Centuries,” *Revista Portuguesa de História*, 18 (1980): 111–31.

²⁷ da Savona, “Breve ragguaglio,” fol. 42.

²⁸ Documents cited and quoted in Jorge Palacios Preciado, *La Trata de negros por Cartagena de Indias* (Tunja, 1973), 349.

²⁹ John K. Thornton, “Demography and History in the Kingdom of Kongo, 1550–1750,” *Journal of African History*, 18 (1977): 507–30. See the revealing comments of Luca da Caltanissetta, “Relatione del Viaggio,” fols. 15, 25v. See the French translation by Bontinck, *Diaire congolaise*, for supporting references to Christianity, especially the rigorous following of the sacraments (particularly marriage) as distinctively Kongolese attributes.

³⁰ For the origin and development of Christianity in Kongo, see John Thornton, “The Development of an African Catholic Church in the Kingdom of Kongo, 1491–1750,” *Journal of African History*, 25 (1984): 147–49.

³¹ Hilton, *Kingdom of Kongo*, 179–98; Wyatt MacGaffey, *Religion and Society in Central Africa: The Bakongo of Lower Zaïre* (Chicago, 1986), 198–211; Andrea da Pavia, “Viaggio Apostolico alla Missione” (MS ca. 1690), MS 3165, Biblioteca Nacional de Madrid; da Savona, “Breve ragguaglio,” *passim*; Rafael Castello de Vide, “Viagem do Congo do Missionário Fr. Raphael de Castello de Vide, hoje Bispo do São Tomé” (MS of 1788), MS Vermelho, 296 fol. 77 and *passim*, Academia das Ciências (Lisbon).

clergy were often absent.³² Even those priests who doubted the orthodoxy of the Kongoleses did not doubt their sincerity, since, as priests, they were often surrounded by crowds of people singing hymns or demanding their children be baptized. The high wooden crosses that marked Kongoleses chapels could still be seen in the twentieth century.³³ These observations by travelers in Africa were seconded by occasional clerical observations in the Americas, where Kongoleses slaves were well known to be Christians.³⁴ The same was true in Spanish Florida, for, in 1748, a free Kongoleses, Miguel Domingo, told the priest at Gracia Real de Santa Teresa de Mose that he had been baptized and continued to pray in Kikongo, probably using prayers like those printed in the Kikongo catechism of 1624.³⁵

In many ways, acceptance of the Portuguese language as the official language of the kingdom of Kongo paralleled the acceptance of Christianity. The author of the account of the Stono Rebellion believed that the slaves were especially open to Spanish propaganda because some could speak its Iberian sister language, and at least some Kongoleses slaves could indeed do this. Almost from the start of European contacts, the Kongoleses developed literacy in Portuguese: the first letter composed by a literate Kongoleses dated from 1491.³⁶ Schools developed rapidly in Kongo, and by the seventeenth century there was a school in every major provincial capital.³⁷ Literacy was more or less restricted to the upper class, but the fact that Kongo's archives and official documents were written in Portuguese helped create a general familiarity with the language among the ordinary people.³⁸

More important for the people, undoubtedly, was the dominance of Portuguese in the language of trade, not just in Kongo but in the whole of west-central Africa.³⁹ The status of Portuguese (or rather, creole Portuguese) as a trade

³² For example, see the account of da Savona, "Breve ragguaglio," fols. 42v, 43, 45, 45v. Also see Castello de Vide, "Viagem do Congo," fols. 53, 64, and *passim*.

³³ François Bontinck, "Les Croix du bois dans l'ancien royaume de Kongo," *Miscellanea historiae pontificiae*, 50 (1983): 199–213.

³⁴ In addition to the correspondence of Antonio de Salas cited above, see John Thornton, "On the Trail of Voodoo: African Christianity in Africa and the Americas," *The Americas*, 44 (1988): 268–69.

³⁵ Landers, "Gracia Real de Santa Teresa de Mose," 27, citing a baptismal register entry of January 26, 1748. The catechism text was reprinted with French translation and annotation by François Bontinck and D. Ndembe Nsasi, *Le Catéchisme kikongo de 1624: Réédition critique* (Brussels, 1978). Italian clergy introduced a new edition in 1650, and copies were extant in the nineteenth century.

³⁶ See Rui da Pina's untitled account (in an early sixteenth-century Italian translation), fol. 99rb–100vb, photographically reproduced in Francisco Leite da Faria, "Uma relação de Rui de Pina sobre o Congo escrita em 1492," *Studia*, 19 (1966), which quotes a letter of the king of Kongo written about September or October 1491. The author of the letter is likely to have been a Kongoleses who had visited Portugal between 1488 and 1491, "a black Christian who knew how to read and write, who began to teach the young men (*moços*) of the Court and children of the nobility, which is a great number"; Rui da Pina, *Cronica del Rey D. Joham I* (ca. 1515), cap. 63, excerpted in António Brásio, ed., *Monumenta Missionaria Africana*, 1st ser., 14 vols. (Lisbon, 1952–81), 1: 136.

³⁷ For a summary of Capuchin educational work, see Saccardo [da Leguzzano], *Congo e Angola*, 1: 402–15.

³⁸ On literacy, see John Thornton, *The Kingdom of Kongo: Civil War and Transition, 1641–1718* (Madison, Wis., 1983), 67–68; Anne Hilton, *The Kingdom of Kongo* (Oxford, 1985), 79–83, 205, 217; Susan H. Broadhead, "Beyond Decline: The Kingdom of the Kongo in the Eighteenth and Nineteenth Centuries," *International Journal of African Historical Studies*, 12 (1979): 633–35.

³⁹ Jean-Luc Vellut, "Relations internationales au moyen-Kwango et d'Angola dans la deuxième moitié du XVIII^e siècle," *Etudes d'histoire africaine*, 1 (1970): 82–89.

language did not mean that all, or even most, Kongolese could speak Portuguese; in fact, governors of Angola complained that even citizens of the colony seemed to prefer African languages to Portuguese at home.⁴⁰ But it did mean that among any sizable group of Kongolese slaves there were likely to be bilingual speakers. During an earlier period, Jesuit missionaries in America had routinely used bilingual Kongolese as catechists.⁴¹ Thus, in the context of colonial South Carolina, Spanish agents were quite likely to be able to communicate with at least some Kongolese slaves, who could in turn communicate with others who knew no Portuguese.

THE SLAVES FOUGHT WELL in the various military engagements that made up the Stono Rebellion. This may have been a result of their bravery and desperation, or they may have acquired military skills as soldiers of the colonies. In the Americas, slaves did sometimes serve in the colonial militia, and they may have been trained there. Wood examined the issue, however, and pointed out that militia service for slaves, while common in the earlier periods, had been phased out by the 1720s. By that time, South Carolina had already passed strong restrictions against slaves possessing firearms, and slaves no longer served in the militia, although many probably still had access to guns for hunting.⁴²

Military service in Africa may well have served a more important role than occasional militia service or hunting. Considering that many slaves were first captured in wars, it is reasonable to assume that some of the rebels had been soldiers.⁴³ Of course, not every person enslaved during military action in Kongo was a soldier. Often, armed forces raided villages, carrying off civilians who would then be sold into slavery. Luca da Caltanisetta, traveling in Kongo, noted one such example that took place on February 10, 1701: "The Mani Lumbo [a title] returned with 58 slaves captured by order of the king; he had destroyed a *libata* [village] of one of his vassals, accused of treason . . . [A]mong these slaves were many free people, some inhabitants of the *libata* . . . and others who were just there on business."⁴⁴ In another incident, da Caltanisetta recorded the poignant tale of a woman who was already a slave but whose master decided to sell her to a trader with connections to the Atlantic trade. Once the woman heard of his plans, she killed her baby and then herself rather than be deported.⁴⁵ Obviously, incidents like these could be multiplied. There were many other ways to become

⁴⁰ This was a constant complaint of mid-eighteenth-century governors; see, for example, Memorial of Francisco Innocencio de Sousa Coutinho, 1765, Fundo Geral, Códice 8554, fol. 28, Biblioteca Nacional de Lisboa. This and other complaints ought to be taken as indications of the failure of the Portuguese government to replace Kimbundu as the language of the colony, rather than that Kimbundu and not Portuguese were known, for Sousa Coutinho took Brazil, a completely Lusophonic country (in his opinion), as a model.

⁴¹ This use had religious implications; see Thornton, "On the Trail of Voodoo," 271–73.

⁴² Wood, *Black Majority*, 127.

⁴³ Wood, *Black Majority*, 126–27, proposed that African soldiers may have been brought to the Americas and cited the use of firearms in Africa, although the evidence relates to the Gold Coast rather than Angola.

⁴⁴ da Caltanisetta, "Relatione del Viaggio," fol. 99v.

⁴⁵ da Caltanisetta, "Relatione del Viaggio," fol. 20v.

enslaved—kidnapping, judicial punishment, or indebtedness. Any of these methods might capture a civilian with no knowledge of military affairs at all. Soldiers were only captured in wars, wars that ranged armies against each other. However, eighteenth-century Kongo had plenty of wars.

Before 1665, Kongo was a centralized kingdom, one in which there was a great deal of internal order. If Kongolese were enslaved, it would be as a result of external attack, not internal disorder; consequently, relatively few Kongolese were enslaved before the mid-seventeenth century.⁴⁶ After 1665, all this changed, as civil wars raged almost constantly for the next forty-four years. While the causes and development of the wars need not concern us here,⁴⁷ it is enough to note that fairly large-scale engagements between Kongolese armies were commonplace. Some of the differences that lay behind the civil wars were patched up when most contenders recognized King Pedro IV between 1709 and 1716 and brought a formal restoration of the kingdom, but intermittent wars broke out after that.⁴⁸ Unfortunately, the documentary record that affords substantial information about Kongo in the late seventeenth and early eighteenth centuries dries up after about 1720. Nevertheless, several incidents in the 1730s are clearly indicative of major wars during that period. For example, a Capuchin priest, Angelo Maria da Polinago, visiting Kongo's coastal principality of Nsoyo in 1733, noted that he was prevented from traveling to the southern areas (in the duchy of Mbamba) because of a major war in that area.⁴⁹ Soldiers captured in this war, or others like it that are not documented, would certainly be exported, via Kabinda to Spanish Cartagena or to South Carolina by the English companies.

Some features of the account of the Stono Rebellion suggest that Kongolese soldiers taken as slaves in this and other, undocumented, wars of the 1730s were among the rebels at Stono. For example, the rebels quickly seized a supply of guns and apparently handled them well.⁵⁰ The utility of guns in a revolt is directly proportional to the skill with which the rebels are capable of using them, and this, in turn, is dependent on training. Presumably, those unfamiliar with guns might have sought other weapons, such as knives, axes, or agricultural tools, and passed up a raid on an armory. Because the colonial militia did not provide much in the way of firearms training, the possibility of an African source of training seems more likely.

Kongolese soldiers would certainly have had training with modern weapons. By the early eighteenth century, guns were becoming more and more common on African battlefields, and skill in their use was being passed on to a larger and larger group of soldiers. A military revolution was altering war in Kongo and other parts of Africa, increasing the size of armies, and replacing the hand-to-hand combat of lances, swords, and axes with the missile combat of muskets.

⁴⁶ Thornton, *Kingdom of Kongo*, xiv–xv. Slaves in the New World known as “Congos” were typically transhipped through the country from farther east and exported either through Kongo ports or by Angola-based merchants who crossed Kongo to buy slaves.

⁴⁷ For a detailed account, see Thornton, *Kingdom of Kongo*, 69–96.

⁴⁸ See Thornton, *Kingdom of Kongo*, 97–113; for a general discussion of the late eighteenth century and the pattern of wars, see Broadhead, “Beyond Decline,” 635–36.

⁴⁹ Angelo Maria da Polinago to Dorotea Sofia di Neoburgo, August 16, 1733, *Raccolta manuscritti*, busta 49, Viaggi, fol. 3, Archivio di Stato, Parma.

⁵⁰ Candler and Northern, “Account of Negroe Insurrection,” 233.

GOOD DESCRIPTIONS OF WAR in seventeenth-century Kongo make clear that, in general, firearms were not important in Angolan wars before 1680.⁵¹ Rather, the musketeers who served were fairly small in number and, moreover, tended to be drawn from an elite of *mestiços*, racially mixed Kongolese with some Portuguese ancestry. At the battle of Mbwila in 1665, Kongo assembled only 360 musketeers under a *mestiço* commander, out of an army of many thousands.⁵² Donigio Carli da Piacenza, an Italian Capuchin traveler, likewise met only some twenty musketeers in the service of the powerful duke of Mbamba in 1668, all *mestiços*.⁵³ Then, such soldiers were not only rare but had social connections that made their enslavement and sale to the Atlantic world improbable.

But this situation changed during the late seventeenth and early eighteenth centuries. In all probability, African generals decided to include more musketeers because of improvements in technology, dating from the 1680s, which saw the replacement of the matchlock musket by the more reliable flintlock weapon. In Europe, armies were altered dramatically; the masses of pikemen who had dominated seventeenth-century wars exchanged their pikes for muskets with fixed bayonets to serve the same purpose. Soldiers armed in this way prevailed over the European battlefield until well into the nineteenth century.⁵⁴ Many African armies seem to have rearmed at about the same time. Ray Kea has shown how armies on the Gold Coast rearmed and swiftly changed battlefield tactics during this period.⁵⁵ Unfortunately, the sources do not allow us to trace developments in Angola with the detail that can be shown in West Africa. Nevertheless, mid-eighteenth-century sources that describe battles fought by the Portuguese-led Angolan army, and its African opponents in southern Kongo, include the musketeer as a prominent force on the battlefield.⁵⁶ He was perhaps significant much earlier. The thousands of soldiers who fell in to greet a Capuchin visitor to Kongo's coastal province of Sonyo in 1694 were already equipped with muskets.⁵⁷ In 1701, Prince António Baretto da Silva of Sonyo felt a sufficient need for firearms that he wrote the pope, asking for a dispensation on the ruling that Prince António not sell slaves to English and Dutch "heretics" in order to arm his soldiers with the muskets they imported.⁵⁸

During the 1730s, however, this military revolution was still incomplete,

⁵¹ For a general survey of the situation, see John Thornton, "The Art of War in Angola, 1595–1680," *Comparative Studies in Society and History*, 30 (1988): 373–75.

⁵² Thornton, *Kingdom of Kongo*, 75.

⁵³ Dionigio Carli da Piacenza, *Viaggio del P. Michelangelo de' Guattini da Reggio et del P. Dionigi de' Carli da Piacenza al regno del Congo* (Reggio, 1671).

⁵⁴ Theodore Ropp, *War in the Modern World* (New York, 1962), 37–60.

⁵⁵ Ray Kea, *Settlements, Trade and Politics on the Seventeenth-Century Gold Coast* (Baltimore, Md., 1982), 154–68.

⁵⁶ Alexandre Elias da Silva Corrêa, *História de Angola* (MS ca. 1789), 2 vols. (Lisbon, 1937), 1: 48–62, a general account of techniques of war in the 1770s illustrating an event (of 1774) based on contemporary records as preserved in a recension of the "Catálogo dos Governadores de Angola." For further textual history, see Joseph C. Miller and John Thornton, "The Chronicle as Source, History and Hagiography: The *Catálogo dos Governadores de Angola*," *Paideuma*, 33 (1987): 359–90.

⁵⁷ Filippo Bernardi da Firenze, "Ragguaglio del Congo, cioè viaggi fatti da' missionarij apostolici cappuccini della provincia di Toscana a' regni del Congo, Angola, Matamba, ecc . . ." (1711), *Archivio Provinciale de' Cappuccini da Provincia di Toscana, Montughi Convent, Florence*, p. 619.

⁵⁸ Prince António Baretto da Silva to Pope Clement XI, October 4, 1701, *Scritture refirite nelli Congressi, Africa-Congo*, vol. 3, fols. 288–88v, *Archivio "De Propaganda Fide."*

especially in areas such as south Kongo, where access to firearms imports was restricted by distance, price, and Portuguese policy. When Anton Felice Tomassi da Cortona visited the duke of Mbamba in Kongo's southern interior in 1734, he found the duke's military retinue still contained archers and lancers as well as numerous musketeers, more than Dionigio Carli had noted fifty years earlier.⁵⁹ Although muskets were not yet the only weapon used, muskets had passed from the prerogative of a small, racially mixed elite to the hands of more people, and the trend toward an increasing use of muskets was clear. By the 1780s, the musket was the most important weapon in use in warfare. In 1781, Father Raphael de Castello de Vide saw the royal army assemble for war, composed, he believed, of "more than thirty thousand men, armed with powder and ball,"⁶⁰ a phrase suggesting a virtually exclusive reliance on muskets. Indeed, other military units that the priest traveled with were armed with muskets as well.⁶¹

Not only were muskets used by a greater percentage of soldiers than before but the use of trained military forces had also spread to outlying areas and led to recruitment of more soldiers among the population. At its height, Kongo's army was centralized: the authorities in the capital (São Salvador) had most of the soldiers, while the provincial nobility possessed relatively small guard units.⁶² But after the civil wars began in Kongo, armed retinues became essential in even small provinces and towns. Tomassi da Cortona noted that the duke of Mbamba had a fairly large standing army in 1734, and his contemporary, Angelo Maria da Polinago, observed that even the small marquisate of Kitombo mustered a considerable force of soldiers to greet him in 1733.⁶³ Thus, although the size of any given army probably fell during the period 1665 to 1740, as a result of the decentralization of power the total number of people under arms and receiving military training with firearms probably rose.⁶⁴

In addition to handling firearms in a way that suggests military training, the Stono rebels also gave other indications of an African military background. For example, they marched under banners like the unit flags that African armies flew in their campaigns,⁶⁵ and they used drums to encourage the rebels. Of course,

⁵⁹ Anton Felice Tomassi da Cortona to his brother, Anibal Tomassi, November 20, 1734, Papers of the Tomassi Family, Cortona, fol. 2. Luca da Caltanissetta also noted that arrows were still used along with muskets in the last years of the seventeenth century; "Relazione del Viaggio," fol. 17 (incident of 1694).

⁶⁰ Castello de Vide, "Viagem do Congo," fol. 118.

⁶¹ Castello de Vide, "Viagem do Congo," fol. 93.

⁶² Thornton, *Kingdom of Kongo*, 42.

⁶³ Tomassi da Cortona to Anibal Tomassi, November 20, 1734, Tomassi Papers, fol. 2; Angelo Maria da Polinago to Dorotea Sofia Neoburgo, August 16, 1733, *Raccolta manoscritti*, busta 49, Viaggi, fol. 2, Archivio di Stato, Parma.

⁶⁴ Miller has argued that firearms were less important in central Africa than proposed here. His main basis for this is the conclusion that firearms imports were insufficient to arm more than about 20,000 soldiers in the whole of central Africa (*Way of Death*, 86–94), even though he estimated that as many as 60,000 guns were imported per year. This low number derives from a pessimistic judgment of the capacity of Africans to repair defective imports or maintain existing weapons. I believe Miller is wrong in part because of the eyewitness evidence of large armies armed with muskets cited in previous notes and also on the grounds that his estimates of imports, number of serviceable weapons at any time, repair capacities of African smiths, and average life span of weapons are all considerably too low.

⁶⁵ Candler and Northern, "Account of Negroe Insurrection," 234. Wood proposed a religious interpretation for the banners, *Black Majority*, 316, n. 30, even though no eighteenth-century source

such behavior might simply have been imitative of colonial militias in which Africans may still have served or at the very least observed. Far more significant was the fact that the rebels danced.⁶⁶ Their dancing need not have been simply a reflection of the joy of the prospect of freedom or the result of drunkenness. Although European and Euro-American armies and militias marched, flew flags, and beat drums as they approached combat, they did not dance. Military dancing was a part of the African culture of war. In African war, dancing was as much a part of military preparation as drill was in Europe. Before 1680, when soldiers fought hand to hand, dancing was a form of training to quicken reflexes and develop parrying skills. Dancing in preparation for war was so common in Kongo that “dancing a war dance” (*sangamento*) was often used as a synonym for “to declare war” in seventeenth-century sources.⁶⁷

Dancing was less useful in the period after 1680, since hand-to-hand combat was largely replaced by missile tactics with muskets. However, Africans did not use bayonets on their muskets; they needed swords and other hand-to-hand weapons for those times when close fighting was required. One Portuguese commander who fought in the late eighteenth century in southern Kongo, after praising his opponents’ skill with muskets and their nearly universal use of the weapon, noted that they retained the “arma blanca” for hand-to-hand fighting.⁶⁸ Thus dancing may have been important even after guns became the principal weapons to ensure that soldiers still honed their skill in hand-to-hand fighting, or it may have survived just as close-order drill survives in modern armies, where it has little combat utility, as a distinctive element of military life. Luca da Caltanissetta still described the musketeer armies of the prince of Nsoyo in 1691 as dancing in preparation for war, and Castello de Vide noted dancing before battle in 1781.⁶⁹

THE AFRICAN MILITARY BACKGROUND can also shed some light on the tactics of the Stono rebels. At first glance, they do not appear to have been very soldierly,

mentions the sort of cultic devotion to flags that he suggested. Unit flags were commonplace, however, in seventeenth-century armies; see Thornton, “Art of War,” 366–67; and Castello de Vide mentioned them as always being a part of any force in the later eighteenth century; de Vide, “Viagem do Congo,” fols. 93, 96.

⁶⁶ Candler and Northern, “Account of Negroe Insurrection,” 234.

⁶⁷ It is so used, for the seventeenth century, even in sources by Europeans; see Mateus Cardoso, “Relação do alevamento de Dom Afonso, irmão del Rey Dom Alvaro III de Congo” (January 1622), *Assistencia Portugal*, vol. 55, fol. 115v, Archivum Romanum Societatis Iesu (Rome).

⁶⁸ Paulo Martins Pinheiro de Lacerda, “Notícia da campanha e paiz do Mosul, que conquistou o Sargento Mor Paulo Martins Pinheiro de Lacerda” (1790–91), *Annaes Marittimos e Coloniais*, 6 (1846): 129–30. Miller, *Way of Death*, 91, n. 57, cited this account as evidence that the Kongo possessed few guns and still fought with their traditional weapons. In fact, he pointedly praised their skill with firearms in a way that suggests it was their principal weapon, noting the use of the “arma blanca” as a secondary weapon. The use of musket fire is also clear in other accounts by Pinheiro de Lacerda, which describe the actual battles of the same campaign in greater detail; see Angola, Caixa 76, doc. 28 (May 20, 1791), Arquivo Histórico Ultramarino (Lisbon); and doc. 34, service record of Felix Xavier Pinheiro de Lacerda (his son), enclosure by Paulo Martins Pinheiro de Lacerda.

⁶⁹ da Caltanissetta, “Relatione del Viaggio,” fols. 9, 25v; Castello de Vide, “Viagem do Congo,” MS Vermelho 296, fols. 98, 119, Academia das Ciências. He also witnessed a *sangamento* in a village, which he compared to these military dances, and was told that the people were in fact dancing to make war on the Devil; fol. 137.

standing in a disorderly group and dispersing after a brief engagement. Many rapidly fled to their masters' homes, and only a small determined core persisted, fighting several more encounters with the colonial militia in the days after the first battle.⁷⁰ It is most likely that, by the time the colonial forces met the rebels, their numbers had been swelled by a large number of slaves who had no military experience but hoped that the rebels would succeed and deliver them from slavery. When the real fighting began, these hangers-on might have been the ones who dispersed quickly, hoping to get back to their masters' farms before they were missed.⁷¹

But the tactics of the core of the rebels, perhaps those twenty Angolans who started the revolt and a few others, who had all the guns, disorderly as they seemed, are consistent with a central African model. Europeans' ideas of a proper military formation were based on the necessity, created by cavalry, of maintaining close order at all times. Indeed, the European musketeer of the eighteenth century was a converted pikeman, just as his musket with bayonet was a combination pike and missile weapon.⁷² In central Africa, where there was never a large or effective cavalry, the musketeer was more likely to be a converted skirmisher. Central African musketeers in the seventeenth century often opened engagements with random fire from covered positions to weaken enemy infantry. This infantry was armed with swords and battle axes intended for hand-to-hand fighting that would resolve the battle.⁷³ But, unlike Europeans, who retained the hand-to-hand aspects of pike warfare, central Africans greatly reduced their close fighting in favor of skirmishing tactics, which replaced the shock encounters of heavily armed infantry. Eighteenth-century battles tended to be drawn-out affairs in which units attacked the enemy, withdrew, maneuvered, and in general avoided hand-to-hand combat.⁷⁴

Thus the Stono rebels were not revealing their rude origins when they fought in the way they did. Instead, their tactical behavior was perfectly consistent with tactics of the battlefields of Kongo. They withdrew after a brief encounter, relocated, and fought several battles over a protracted period, a pattern typical of Angola.

We can see the Stono Rebellion from a new angle if we consider the African contribution as well as the American one. The combination of evidence certainly suggests that the slaves' Christianity and the religious appeal of Spanish propaganda may have played a role in the revolt. Likewise, though less certain, the slaves' probable military experience in Africa could also have influenced their behavior and their ultimate fate.

⁷⁰ Documents summarized in Wood, *Black Majority*, 318–20.

⁷¹ Candler and Northern, "Account of Negroe Insurrection," 235.

⁷² William H. McNeill, *The Pursuit of Power: Technology, Armed Force and Society since 1000* (Chicago, 1982), 125–39. When fighting with Native Americans, and even during the colonial wars, colonial militias sometimes abandoned these principles, however, much to the chagrin of their professional leaders or opponents.

⁷³ Thornton, "Art of War," 363, 374.

⁷⁴ The best description involves a southern Kongo army fighting Portuguese-led forces in the mid-eighteenth century; Silva Corrêa, *História de Angola*, 2: 48–62. See the interesting description of tactics in Pinheiro de Lacerda, "Notícia da campanha," 131–32.



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This year for the first time, a sizable portion of the film section is devoted to the history of a single country, Germany, and, more specifically, to the Third Reich and its legacy. The aim of this subsection is twofold: to begin to look beyond the boundaries of the individual work of history on film in order to glimpse the broader historical realm that—like the realm of written history—is created when a number of filmmakers work on a single topic or period of the past; and to see how this realm, in its topics, issues, data, interpretation, and historical strategies, relates to the more familiar world of history that we encounter on the printed page.

For this sort of attempt to explore history on film, one could find no richer topic—even before reunification made the issue pressing—than modern Germany. This is not just because, for fifty years, the Third Reich has been a staple screen subject for European and American filmmakers but, rather, because so many German filmmakers have in the last two decades found it necessary to wrestle with the meaning of their nation's past. The focus of this section is, indeed, on how German filmmakers have come to grips with the legacy of a regime that, more than any other in history, unscrupulously and systematically misused motion picture images as vehicles for lies and propaganda. One result has been that probably in no other country have filmmakers engaged in such an effort to innovate in film, both to find fresh images and to pursue new strategies that will help create a usable past.

Four of the six German films reviewed here, all directed by well-known filmmakers associated with the New German Cinema, were dealt with in a recent brilliant study, *From Hitler to Heimat: The Return of History as Film* (Cambridge, Mass., 1988). Author Anton Kaes, a professor of literature, claims that these works not only reflect a broad range of contemporary approaches to history (*Alltagsgeschichte*, women's history, and social history) but also that they anticipated the arguments of the recent *Historikerstreit* by a decade. To evaluate their contribution from the perspective of the historian, we have specialists review the following: Hans-Jürgen Syberberg, *Hitler—A Film from Germany*; Edgar Reitz, *Heimat*; Helma Sanders-Brahms, *Germany, Pale Mother*; and Rainer Werner Fassbinder, *The Marriage of Maria Braun*. To these, two other films have been added: *Deutschland im Herbst*, a collective endeavor from 1977 that turned a number of German directors toward an interest in their history, and Michael Verhoeven's *Nasty Girl*, a recent film that has not only managed to reach a popular audience both in and outside of Germany but is one of the few films to deal with the legacy of the Third Reich in terms at once ironic, humorous, and serious.

The focus on German history was planned, but two other themes that run through the section emerged spontaneously as reviewers attempted to evaluate complex works of visual history: one is the problematic relationship between memory and history, and the other is the historical film as a contribution to historiography, that is, to the way we think about the past. Both themes are clearly important for the question of how and in what ways film can contribute to historical knowledge and understanding.

The relationship between history and memory is central to that most common genre of

documentary that creates a historical world by playing the words of talking head “witnesses” (people who lived through events) off against the voice of history as enunciated either by experts (historians) or an unseen narrator. In *History and Memory*, Rea Tajiri makes this relationship central to a portrait of her Japanese-American family’s internment in relocation/concentration camps during World War II, prior to her own birth. For Tajiri, the issue is, in part, one of erasure: how to recapture and evaluate all those repressed events too painful for people to remember. Or the corollary: how to deal on film with those remembered events that did not take place in front of a camera. A reverse question runs through Syberberg’s *Hitler*: How can the filmmaker treat the footage of events that, like many Nazi rallies, were largely staged for the benefit of the camera? Which is to say: How should one utilize in history the authentic image that was compromised at its source, the true image that perpetuates a lie?

Other questions concerning history and memory might be asked of the talking heads that carry the burden of meaning in many documentaries. For example, which of the witnesses in *Berkeley in the Sixties* are we to believe, and on what grounds—how well they have aged? what they wear? what we can glean of their current occupations? Do their multiple, conflicting testimonies add up to history? Similarly, to what extent do the well-trained voices of famed actors reading mid-nineteenth-century letters, memoirs, and speeches in *The Civil War* contemporize, domesticate, or romanticize events of that era? Does a narrator like the charming Shelby Foote turn potentially critical issues into sentimental memories? And is such sentiment what we want from the past?

The question of what is repressed and what remembered, personally and officially, is hardly absent from dramatic features. It hovers over all films dealing with the Third Reich and is central to *The Nasty Girl*, which at the outset depicts workmen painting over the following graffiti on a public wall: “Where were you between ’39 and ’45?” It also marks the treatment of the consequences of the atomic bombing of Hiroshima in *Black Rain*, which tells its story in terms of the difference between personal experience and public event, pointing to the variety of different histories and senses of time that simultaneously exist in any complex culture, as if daring us to choose one and call it the definitive rendition of the past.

Black Rain carries us into the issue of film as historiography, for it raises the question of who gets to write history and who does not, what data gets included and what erased, and what notion of time (circular or linear) underlies our idea of the past. Questions of time itself as variable, along with the determination of what constitutes suitable data, inform Syberberg’s *Hitler*, which thus becomes a critique of current historical practices. In a similar way, both *Deutschland im Herbst* and *Un Médicin des Lumières* may be less important for delivering “true” data than for forcing the audience to consider the historical categories and assumptions framing that data. If the reviewers are right, this would mean that film can interact with written history by raising questions we professional historians might feel obliged to answer.

In certain pressing situations of social change, theoretical questions about the relationship between history and memory or the possibility of film as historiography may seem like the luxury of those who live outside history. Certainly in Eastern Europe or the Soviet Union today, the issue for the historian filmmaker is more basic: how to dig up facts that have been long suppressed and tell them to the largest possible audience. For the directors whose works are part of the Glasnost Film Festival, desperate to reveal the hidden past, information is all. The same is true for those American historians whose fortuitous arrival in the midst of the Donetsk miners’ strike allowed them to produce a work of history-in-the-making, the startlingly direct *Perestroika from Below*. The record of such momentous events, captured on film, is a good reminder that history in whatever medium cannot be divorced from empirical data, the evidence from the past that the filmmaker, like the historian who works in words, must shape into a record that allows us to understand the moments and processes of social, political, and cultural continuity and change.

Of Further Interest: to pursue the topic of how German filmmakers depict the Third Reich, one could begin with such standard works on the New German Cinema as Klaus Phillips, ed., *New German Filmmakers* (New York, 1984); John Sanford, *The New German Cinema* (New York, 1980); and Thomas Elsaesser, *New German Cinema* (New Brunswick, N.J., 1989). But the best place to begin is with Kaes, *From Hitler to Heimat*, supplemented by Annette Insdorf, *Indelible Shadows: Film and the Holocaust*, 2d edn. (New York, 1989), especially chapter 11, "The New German Guilt."

German films that deal with the history or legacy of the Third Reich comprise a long list; of those not reviewed in this section, one would certainly include as among the best the following dramatic features (all distributors can be found in Insdorf's filmography): Volker Schlöndorff, *The Tin Drum* (1979); Alexander Kluge, *Die Patriotin* (1979); Peter Lilienthal, *David* (1979); Rainer Maria Fassbinder, *Lola* (1981), *Veronika Voss* (1982), and *Lili Marleen* (1981); and Jeanine Meerapfel, *Malou* (1982). Most German documentaries on the topic (mentioned by Insdorf) are difficult to obtain in the United States, but one excellent available work is Harald Lüders and Pavel Schnabel, *Now . . . After All These Years* (1981).

For the legacy of Nazism in Austria, foreign filmmakers seem to have done better than native ones. Most impressive is the dramatic trilogy by French-born director Axel Corti, *Where to and Back*. Based on a semi-autobiographical screenplay by Georg Stefan Troller, the work consists of *God Does Not Believe in Us Anymore* (1981), *Santa Fe* (1985), and *Welcome in Vienna* (1986). For documentaries, one can learn a good deal about how the Viennese view their experience of the Third Reich on the fiftieth anniversary of *Kristallnacht* from *Vienna Is Different* (1989), by American directors Susan Korda and David Leitner (distributed by Filmmakers Library, 124 E. 40th St., N.Y., N.Y. 10016).

ASIA

Black Rain. Directed by Imamura Shohei. 1988; black and white; 120 minutes. Japanese with English subtitles. Distributor: Angelika Films.

In a scene near the start of *Black Rain*, a man, blinded by the world's first atomic attack, looking over but not seeing the city, cries out "Where's Hiroshima? It's all disappeared." Later, a survivor of the bomb accuses everyone of having "forgotten how Hiroshima and Nagasaki were destroyed." These two scenes, which cast experience as blindness and pose witnessing against forgetting, speak to the film's central concerns. *Black Rain* raises questions about how we experience the historical event, how we remember, and who is authorized to do the remembering.

Black Rain situates itself at two points in time. The opening minutes of the film and the flashbacks in its first hour recount the atomic attack and its aftermath, portraying the Shizuma family's escape, almost a journey of pilgrimage, through a city reduced to rubble and flames and peopled by corpses. The remainder of the narrative unfolds five years later. It chronicles six months in the life of an agricultural community some distance from Hiroshima, a community presided over by the Shizumas in their guise as local landowners.

Cutting between 1945 and 1950, *Black Rain* negotiates between alternative modes of placing the self in time: between, on the one hand, a narrative of catastrophe, in which the atomic explosion constitutes a singular, dramatic event, and, on the other hand, a structure in

which the salient fact about human experience is that it lacks events, that it repeats and perpetuates "the same." Near the beginning of the film, we see a clock in a Hiroshima railway station blown out of its frame at the moment (8:15 a.m.) of the atomic blast, and in the next sequence we see passengers in a boat salvage the wreckage of another clock, also stopped at 8:15, from the waters of Hiroshima Bay. In 1950, in the village of Kobatake, the Shizuma household does not stop time but lives by the dictates of cyclical time. The household takes its evening meal at the same hour each day, while Yasuko, the niece, sets the clock by the radio news: 7 o'clock comes around repeatedly, no matter what the news is, no matter how history is unfolding on, for instance, the Korean Peninsula, a place that by the film's end America views as a fitting target for another A-bomb.

To some extent, *Black Rain* is concerned with how these different temporalities intermingle. The sequences in 1950 are punctuated by the slapstick comedy that ensues every time a car passes through Kobatake and every time the village idiot mistakes the vehicle for a tank and stops it in its tracks. Yuichi's one-man army is a diversion on the route through Kobatake that travelers come to expect. His regularly scheduled hallucinations assimilate the past event to the present routine. By 1950, the nuclear attack has also been rendered familiar, and familial. It becomes a structure that, like the family, provides a ground for individual identity. Thus Imamura's heroine, Yasuko, refuses to rejoin her father's family and leave her uncle and aunt because the three of them constitute "a community bound by the Atomic Bomb."

It is hard to say how one is meant to react to the ease with which these survivors make past trauma routine. Constructing a "people's history," Imamura seems both to admire the resilience of ordinary people and to condemn them for acquiescing in their role as those who look on while history happens, as Yasuko and her uncle do in a sequence in which an anti-nuclear rally passes them by, unheeded. In the main, however, the film's ambiguity lies in the way Imamura portrays the Japanese social order as working with the same oppositions between the event and the uneventful that the crosscutting and the repetitions structuring his film seem to challenge. The latter half of *Black Rain* turns on how the power structures that disallow women's claims to consequence intersect with these different temporalities and exploit the opposition between them; it demonstrates how the reproduction of social inequality depends on producing histories in one mode at the cost of the other.

Women in the world of *Black Rain* are associated, as in most patriarchal cultures, with the uneventful. The domestic labor that claims the energies of Imamura's female characters long epitomized the ahistorical for historians, precisely because such housework is never done. If history is conceived of as a narrative of events, the community of Kobatake is outside history: it is a place where women labor and old men rest, and young men (among them prospective husbands for Yasuko) merely visit. An analogous conjunction of the feminine and the changeless is made through the character of Yasuko, who, as she is told repeatedly, is the "very image of" her deceased mother. In the voice-over narration that starts *Black Rain*, Yasuko lists the household goods she was conveying from Hiroshima the morning of the bomb—her aunt's kimono, great-grandmother's wedding gown, uncle's formal wear, her own belongings. The voice-over sketches a concept of family life as the prime locus for cultural continuity: a concept of women's work as the foundation of a domestic structure that persists despite change and that, in persisting, allows individuals to extend their selfhood through the generations.

The 1950 sequences of *Black Rain* trace how Yasuko suffers because the event of August 1945 has called into question her relationship to the cyclical time of the domestic sphere and to the standard narrative of Japanese womanhood—the passage from schoolgirl to bride to mother. These sequences demonstrate likewise that Yasuko suffers because her claims that the history of the A-bomb is *her* history go unacknowledged. The "historical" knowledge the parents of her suitors possess—their knowledge of her presence in Hiroshima on August 6 and their surmise that she is sick or sterile because of radiation exposure—prevents Yasuko from marrying. The knowledge stops her from getting on with life and out of history. At the same time, her uncle's insistence that his niece's life story has nothing to do with the bomb, that she was in Furue not Hiroshima, that she merely encountered "the black rain" that followed the

bomb blast, and that she was not, strictly speaking, present at the event, discredits Yasuko's sense of self. Shizuma will not recognize that the question of her marriage is no longer relevant to Yasuko; that Yasuko no longer thinks of herself as belonging to the category of people who marry.

Black Rain, then, is not only the story of Hiroshima's annihilation, it also traces the erasure of Yasuko's story. Yasuko's voice-over is superseded in the film by Shizuma's narration, which reconstructs August 6, 1945, from his point of view. Yasuko never reoccupies the position of knowledge she holds at the film's start, a position in which she is the historian. Indeed, she ends up dispossessed of the diary (*her* diary) from which she reads in the voice-over. To "prove" his niece healthy and to advance his plans to marry her off, Shizuma obtains an official medical certificate and supplements it with recopied excerpts from Yasuko's diary. He composes, in short, a history that, in the best tradition of the discipline, is made up from eyewitness accounts and expert testimony. Ironically, the time that Shizuma spends compiling this history coincides with the time that Yasuko falls ill from radiation sickness.

Much as doctors' cures for radiation sickness are posed in the film against women's folk remedies, Shizuma's authorized history is posed against not only the history told by rumor but also the history written when the "black rain" ("black as ink," according to Yasuko's diary) falls on Yasuko—a history that is finally rewritten by the marks that inscribe the bodily surfaces of the victim of radiation sickness. *Black Rain* makes visible what must be overlooked to ensure the "truth" of history: histories of the body, of the voices of gossips, of different kinds of witnessing and remembering.

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AUSTRALIA

Cane Toads: An Unnatural History. Produced by Tristram Miall; directed by Mark Lewis. 1987; color; 46 minutes. Distributor: First Run Features.

An exterior shot of a suburban house at night. Once inside, the camera reveals a bathroom, where a man sings in the shower. The scene is full of familiar suspense: the threat of something unseen, something approaching that we and he cannot see. The man looks and the camera turns, revealing a large khaki toad. The acerbic brilliance of *Cane Toads* is to combine serious ecological history, witty cultural observation, and cinematic sophistication. It is not the combination itself that makes this documentary a fine history but how it is achieved: it is a matter of style.

At the center of the film is an apparently simple story, concerning a now-widespread exotic pest, the toad *Bufo marinus*, in Queensland. The toad is highly adaptable, voracious, omnivorous, and fatally toxic to would-be predators. Its appetite for food is matched by its appetite for sex; it breeds quickly and in huge numbers. Sixty years after its first appearance in Queensland's tropical coastal wetlands, the toad, now regarded as an "amphibian weed," has invaded the ecology and endangered native species.

Bufo was introduced in order to control cane beetles, which threatened to destroy Queensland's sugar cane industry in the 1930s. These beetles were native to Queensland's

rainforest, and, when the rainforest was replaced by the cane field, they developed a taste for cane. The immature grub was the cause of the problem: it burrowed into the soil around the cane and ate the plant's roots, killing it. A bad grub attack could reduce a farm's yield by 90 percent. It seemed nothing could stop the grubs, until the entomologist Raquel Dexter, in a paper delivered in 1932 to the Puerto Rico congress of the International Society of Sugar Cane Technologists, suggested that toads might be the solution. Believing that the toads would eat only the beetles and other night-flying insects, the Queensland Bureau of Sugar Experiment Stations introduced them to a number of sites in 1935. They had no effect on cane beetles, for a simple reason. Whereas the beetles lived in the cane, the toads lived on the ground. Their paths did not cross. Eventually, the beetles were controlled by a chemical poison, benzene hexachloride, but the ravenous *Bufo* had long turned its attention elsewhere. An early indication of its destructive potential was the disappearance of stray dogs around the release sites. The dogs were the first of many animals poisoned attempting to eat the toads.

The toad has inevitably become an abundant resource for Queensland culture. Just as the toad has adapted and exploited its new environment, so people have adapted to the toad, finding uses for it, integrating it into folklore and everyday life. The film shows a small girl hugging and kissing her pet toad, the hideous "Dairy Queen." A local politician discusses his plan to build a small monument to the cane toad for the Australian bicentennial of 1988. The monument, he explains, would "record a unique part of north Australia's history" and attract tourists. A cane farmer, one of many Queenslanders who exterminate toads at every opportunity, responds: "Surely this bastard can't be in his right frame of mind. Fancy anyone wanting to erect a monument or stone or something to this creature who had been brought in." The toad is both the quintessential alien and a highly ambiguous symbol of regional identity. *Cane Toads* attends to every irony and nuance in these processes of culture.

The film is organized as a series of titled sections, which scrutinize different aspects of the cane toad phenomenon: the toad's sexual habits, its poison, its domestication, and its breeding and eating habits. The resemblance between this expository structure and traditional forms of natural history writing is not coincidental. *Cane Toads* aims to show us how the toad works. It takes its object to be something of inherent interest, and it is never tempted to make the toad or its depredations metaphors for other things (see Albert Moran's excellent commentary, "Multiculturalism, Ecology and the Invasion of the Body Snatchers," in J. Dawson and B. Molloy, eds., *Queensland Images in Film and Television* [Brisbane, 1991]). Thus the toad is always the central character, and it is this insistent focus that is the key to the film's considerable stylistic achievement.

Cane Toads carries off innumerable jokes and exploits to the full a distinctive palette of stylistic effects—including the scene-setting devices of the gothic, alien, thriller, and horror genres—without ever distracting itself from the central purpose of describing and explaining the toad phenomenon. An audience might interpret the film's use of genre—as in the contrivance of the shower scene—as a suggestion that the toad is like the monstrous aliens of cinematic fantasy. *Cane Toads* could then be appreciated as a postmodern documentary, yet another film about film. But such an interpretation would not be consistent with the film's emphatic argument for a specific understanding of the toad and its dangerous potential. Instead, the film plays on a different relationship between the particular and the general: it proposes that genre aliens are like cane toads or, at least, that alien films help us understand how people think about cane toads. What might seem a tangential flourish actually has the effect of fixing our attention once again on the toad, while telling us something more about the toad's engagement with culture. It is this dedication to describing and explaining the particular that makes *Cane Toads* an exemplary film history.

Just as the film shows us how people adapt to the cane toad, it also demonstrates how easily film accommodates the toad. The toad is as comfortable in front of the cameras as it is anywhere else. *Cane Toads* is an exercise in cinematic adaptation. It perfectly illustrates the toad's resourceful domination of a new environment. From the point of view of the cane toad, of course, this is a triumph. Having occupied more than half the east coast of Australia, the toad

has now secured a foothold in film. As before, the consequences can only be guessed, and further incursions are inevitable. As the song goes:

Cane toads are coming, cane toads are coming
Main roads are humming with the cane toad blues . . .
When are you going to leave me alone?

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EUROPE

Un Médecin des Lumières. Produced by Antenne 2; directed by René Allio. 1988; color; 270 minutes.

La Révolution française, I: *Les Années lumières*. Produced by Ariane-films, Antenne 2; directed by Robert Enrico. 1989; 120 minutes; II: *Les Années terribles*. Directed by Richard Effron. 1989; color; 120 min.

The encounter was unavoidable: since France decided to pompously celebrate the bicentennial of its revolution—actually, the bicentennial of the first year of the revolution—history flooded the screens in 1989. Some 120 hours of documentaries, dramatizations, and lectures dealing with the period were programed in theaters or on television. A good many pictures took the revolution as an exciting background for a thriller or a love story, and it is less their content than their number that is significant: for nearly two centuries, the revolution, celebrated by the left and condemned by the right, had been the watershed of French political life. This meant that filmmakers who wanted to lure big audiences carefully avoided the topic. The fact that more films on the revolution were made in 1988–1989 than during the previous decades of the century means that 1789 is no longer the dividing line of French history.

For this review, I have selected two different works likely to illustrate the potentialities and shortcomings of screened history: a television miniseries, *Un Médecin des Lumières* (A Physician of the Enlightenment), and an expensive film production, *La Révolution française*. The miniseries, which is set in the fifteen years preceding the revolution, was purposely shown on television July 9, 1988, as a prelude to the bicentennial. It is what might be called a case study: a young doctor, Simon Bertiny, settles in Bourbonnais, a rural, isolated countryside, where he tries to introduce ideas and methods borrowed from the Enlightenment. The big production was originally planned as a national event, a series of four two-hour movies released at the end of 1989 to close the celebration. For financial reasons, it was reduced to only two pictures; the first met with a reasonable response, while the second was not a success. It would be interesting to know whether people enjoyed the first because it depicted the “happy” early years of the revolution but avoided the second because it depicted the Terror, or whether, perhaps, by the time the film came out, the French audience was merely fed up, having already seen too much of Louis XVI, Danton, and Robespierre.

René Allio, a well-known film director, shot the miniseries, *Médecin*, like a film, while the two directors of *La Révolution* were obviously influenced by the prospect of having their work shown on television. Indeed, stylistically, *La Révolution* belongs to that new breed of historical movies shot and edited as if they were compilations of telenews. This is not meant as a criticism.

On the contrary, it might be argued that spectators today can better understand what was happening in 1789 if the king is filmed as if he were a contemporary American president.

In order to underline the tele-visual influence on *La Révolution*, let me point to three of its features. First, in a "classic" film, actors, when conversing, look at each other or at the center of the circle they form. *La Révolution* shoots them full-face; they talk for an unseen mike and behave like politicians being interviewed. Second, most shots, especially the long ones, are taken in wide angle; this makes the picture sharp everywhere, but it also makes it difficult to follow any detail or particular action, with the result that spectators are offered more passing impressions than precise data. It must be added that, apart from the leading figures, the film does not identify any character. We meet neither the craftsman nor the clerk who resurface every now and then to express the feelings of their comrades. From time to time, we spot a face we have already seen, but it is a nameless face, one that makes us understand that many anonymous individuals were involved in the event from beginning to end. Third, the editor has heavily intercut the scenes, except those with dialogue. The attack on the Bastille is worth mentioning in this respect—a series of short pictures present us with people running through the dust, shouting, changing direction; everything is in turmoil, which, we later learn, turns out to be a political upheaval.

The two parts of *La Révolution* play with both a well-known story and a contemporary way of filming. Yet neither part makes them work together successfully, and their failure has much to tell us about the representation of history. So many books have dealt with the revolution that it is hard to escape the formula that begins with a description of the *ancien régime*, introduces the main actors, and confines them to a limited cluster of unavoidable events. *La Révolution* sticks to the established model, which means it is nothing but an illustrated textbook. The television-like style suitable for short, thrilling scenes such as the Bastille no longer works when the characters explain what is happening. Obligated to speak at length, the historical figures begin to look like what they are, actors. We do not really believe that Louis XVI has been interviewed, for he says what our books have taught us he ought to say and not what a politician would hurriedly deliver in front of a television camera.

All this makes me think that cinema or television cannot effectively compete with written accounts about a well-known problem or period: the dominant historical scheme, conceived by writers, is not fit for the screen. Take a commonplace of the traditional narrative, the anger and frustration of the bourgeoisie: how is it possible to express these feelings visually? Films are reduced to giving examples. At the beginning of *Les Années lumières*, a young, poor pupil, Robespierre, is entrusted with welcoming the king to his school in Arras; neither Louis XVI nor his court cares for the boy, who feels deeply humiliated. But a great many bourgeois had been humiliated in the past yet did not overturn the government, and it is misleading to suggest that Robespierre did what he did because of this single incident. Social conflicts cannot be reduced to a personal grudge.

Still, it seems difficult to ban stereotypes totally, since even a film as innovative as *Un Médecin* indulges in conventions, re-uses vignettes pictured in many previous productions (for instance, the eviction of a destitute tenant), and credits its main character, Dr. Bertiny, with the inevitable affair with the squire's wife. These clichés could be avoided in a didactic program made especially for students, but a filmmaker involved in a television series does not dare omit them. Oddly enough, such clichés also help Allio introduce a surprising vision of the *ancien régime*. We expect the squire to be unfair, to exploit landholders, to try to reinforce his power over the village; we are also prepared for a clash between him and Bertiny. But, in this film, poverty and injustice do not seem to lead to revolt, and the peasants passively accept what their fathers have endured before them. Since there is no town in the district, there is no struggle between the main landowner and merchants or clerks intent on supplanting him.

Bertiny himself, imbued though he is with "philosophic" principles, does not think in political terms. He is more committed to improving the countryside's daily life than to reforming institutions. The director's images have been cleverly devised to illustrate the distance between the physician and his patients; despite his generosity, Bertiny sees them from outside, a vantage point that enables him to take notice of their infirmities and to relate them

to their living conditions. Bertiny is a disciple of science, not a rebel. He gets on very well with the parish priest, who shares his concerns. If he quarrels with the squire, it is only because this stubborn petty noble fears any external influence. Healthier workers would make the land more profitable, but all ideas are revolutionary for the landowner because they can introduce changes and threaten traditions.

We know of historical experiences similar to Bertiny's, but it would be difficult to document fully a four-hour production on this topic. Hence the filmmaker has chosen to give us this plausible fiction to describe the past. Is it possible for us historians to accept such a modern invention as a piece of history? I think it is, at least with regard to an issue such as this on which people have preconceptions. *Un Médecin* substantiates various aspects of the *ancien régime*—agricultural backwardness, seigneurial power, and illiteracy—but questions the dominant vision that a social crisis led inevitably to an uprising. In doing this, the film encourages viewers to reopen the debate on the origins of the French Revolution. Such a work is highly valuable not because it is “true” but because it is a piece of historiography.

Pierre Sorlin

The Sorbonne

Hitler: A Film from Germany. (Released in the United States as *Our Hitler*). Produced by TMS/Solaris/Westdeutscher Rundfunk/Ina/British Broadcasting Corporation; written and directed by Hans-Jürgen Syberberg. 1977; color; 6 hours, 45 minutes. German with English subtitles and English. Video distributor: West Glen Films, 1430 Broadway, New York, N.Y. 10018.

A visual and aural pastiche portraying “our” Hitler, Hans-Jürgen Syberberg’s nearly seven-hour film has been an international scandal from the moment it appeared. *Hitler* culminates a Syberbergian “trilogy” that includes films on the Bavarian King Ludwig II and the German writer of adolescent adventure stories, Karl May, among others. Like these films, but even more controversially, *Hitler* deals with a historical subject by dissolving it as history.

On one level, the film is a fascinating document of 1970s postmodern cinema. Syberberg eschews standard historical narrative, uses documentary footage in unorthodox ways, deploys no interviews or location shots, makes provocative juxtapositions (Nazi judges, the heads of major Hollywood studios, and the Russian director Eisenstein’s enemies, for example), and interweaves recordings of historic German radio broadcasts with Wagner, Mozart, and popular music. The film is not without structure, but its winding, nonlinear, and associative technique gives it several beginnings, middles, and ends. Its sheer excess, stemming partly from Syberberg’s goal of creating a cinematic “total work of art,” is more likely to engender fatigue than critical reflection. And yet, although *Hitler* laboriously rejects the sort of relationship with the past that a historical monograph or a documentary establishes, it invites speculation on what use the historian can make of it.

The film is divided into four parts, whose titles suggest its texture: “Hitler: A Film from Germany (From the World Ash Tree to the Goethe Oak of Buchenwald)”; “A German Dream (. . . Until the End of the World)”; “The End of a Winter’s Tale (And the Final Victory of Progress)”; and “We Children of Hell (Recall the Age of the Grail).” It begins and ends with scenes of outer space, a device many critics have attacked because it allegedly trivializes historical events. Within this frame of cosmic endlessness, Syberberg digresses and backtracks, creating a mournful atmosphere punctuated by bizarre humor that only accentuates the darkness. All the action, if that is what it can be called, occurs on a stage where actors perform

monologues as they project themselves onto Hitler, "the German Napoleon of the twentieth century," who is "more of a non-person," as the circus master of ceremonies played by Heinz Schubert says early in Part One. Hitler is an empty center, a cinematic and historical absence that various actors, portraying historical as well as fictional characters, fill with their own memories, fantasies, and anxieties. Schubert speaks the key lines: "Let's give him his chance, let's give ourselves our chance."

What follows is a multilayered intertext of Richard Wagner and Bertolt Brecht, kitsch and colportage, pornographic and Hollywood imagery, fragments of Hitler's recorded speeches and other German radio broadcasts, commentaries on absurd historical trash ("gougued-out eyes of SS men" and the replica of a canister of Hitler's sperm "on the model of the original sperm bank in Hollywood"), background projections of famous paintings and newsreel footage, and much classical music. All this is interwoven with the actors' monologues, which are sometimes spoken to or through puppets of Ludwig II, Hitler (complete with bloody bullet hole in his head), Eva Braun, and Nazi officials Joseph Goebbels, Hermann Goering, Heinrich Himmler, and Albert Speer. There are disturbing scenes: an SA man (played by Peter Kern) reciting the murderer's final monologue from "M," Hitler (Schubert) wearing a Roman toga while arising from Wagner's grave as Wagner's *Rienzi* is played in the background, and Himmler (Schubert again) revealing his innermost thoughts to his masseuse while actors playing SS men recount tales of extermination.

Saul Friedländer has criticized this dispersed cascade of images in *Reflections of Nazism: An Essay on Kitsch and Death* (1984), arguing that it distorts the historical record and offers aesthetic excess where modesty should prevail. There is something to the criticism. But *Hitler* can also be interpreted in a spirit of earnest playfulness, an unusual and risky position in modern German history, where moral stakes are very high, and where belief in writing history "as it really was" is still strong. The risk is worth taking if we stress the seriousness of play and the potential it offers—as a first step—for redescribing a tragic history.

To think of *Hitler* as more than a curiosity or an artistic document is to give Syberberg the chance to "get the story crooked," to use Hans Kellner's telling phrase on written history in *Language and Historical Representation* (1989). But what story is being made crooked? Certainly not the one of the historical Hitler, at least not in any direct way. Syberberg argues there are still powerful desires (for myth, intimacy, wholeness, oblivion) that feed "our" Hitler just (though not quite) as they fed the historical Hitler. He and several commentators have called the film a work of mourning that takes leave of such desires but returns to them, chastened, under the sign of postmodernity. It is doubtful that any written text could deal with this issue as provocatively or (at times) as movingly as Syberberg does. *Hitler* is about deep processes of identification, projection, and representation. Syberberg's focus on "our" (Germans'? cinema audiences'? the West's?) Hitler indicates that the director has taken self-reflexivity to an extreme, allowing his mournful musings about identity and representation to subsume the historical subject in a space of poetic imagination.

Most historians of the Nazis will not want to go this far, but they will find in *Hitler* an upper limit for constructing a self-referential story of their own strategies of representation, the mirrors they use to construct the past. Getting *this* story crooked would entail an internal critique of key features of German historical studies: reliance on the nation-state as the basic unit of analysis, the dichotomization of reason and unreason, the continued personalization of Nazism, a reluctance to admit the "literariness" of historical research, and the absence of systematic discussion of the way in which historians identify with their subjects.

It would also mean a reconsideration of visual images. The historical record of Nazism has been shaped by such imagery, partly because the Nazis used filmic resources more than previous political regimes had, giving us endless scenes of fluttering banners, marching SA units, and mesmerized audiences. Syberberg "misuses" newsreel footage and other historical materials in order to critique these images, arguing that their origins and manipulation by mass media make conventional scholarly use of them impossible if not illicit—his "Hitler" is a counter-discourse to the "Hitler" that has become "a film."

Historians can use this critique as a reminder of the need to consider the "worklike" as well

as documentary aspects of “authentic” historical images they often take for granted. If, today, so much of Nazism is a picture supplemented by words, and if that picture is viewed not as an unproblematic document but as a result of the perpetrators’ self-presentation, then historians ought to be as careful in their use of these sources as Syberberg is ruthless in manipulating them. But this prompts a quite different question, which can only be stated rhetorically: what kinds of visual images would a specialist in the field choose if given the chance to construct a “Hitler” without these usual filmic texts or without recourse to standard documentary or dramatic methods of deploying them?

There is obviously much more to be said about this extraordinary work—its use of memory and myth, its elegiac aesthetic, its submerged historical narratives, its potential for discussing the “ambience” of Nazism. More than most films—and with an incomparable amount of hyperbole—Syberberg’s *Hitler* raises questions not just about German history but about the possibilities of historical representation, filmic and written. The film deserves more sympathetic and thorough consideration by historians than it has had so far.

Rudy Koshar

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Heimat: Eine deutsche Chronik. Directed by Edgar Reitz. 1984; color; 15 hours, 21 minutes. German with English subtitles. Video distributor: West Glen Films.

*H*eimat, which refers to homeland or roots, is a fifteen-hour film written by Edgar Reitz and Peter Steinbach, a haunting eleven-part epic tracing three generations in the remote Hunsrück village of Schabbach. *Heimat* is also a film about history, ostensibly, an attempt to view Germany from 1919 to 1982 through the lives of two farming families, the Simons and the Wiegands.

Clearly influenced by the new social history, the authors take as their premise the idea that the past can only be resurrected at the local or vernacular level. Most of the film’s action takes place in Schabbach or the nearby small town of Simmern. Village life is altered more by the introduction of television and telephones than by elections or political events in Berlin. This approach leads to great concreteness. The Nazis are not faceless, uniformed marching soldiers but Eduard, the bumbling Simon boy, who is convinced there is gold in the village stream, and Wilfried, the cruel Wiegand son who coolly shoots a seriously injured British pilot. The authors slight all events that, in their view of the *Alltag* (everyday life), had little impact on provincial Schabbach. The film tells us little about the Kapp *Putsch*, inflation of 1923, depression of 1929–1933, mass slaughter of Jews, or, for that matter, Turkish guest workers. This is interpretation with a vengeance. In an interview with *The New York Times*, Reitz said, “Schabbach is good because it is so small, and because it is small it is innocent.” He expressed his disgust at Hollywood films about Germany as an attempt to colonize the German mind.

In the opening scene, young Paul Simon returns to Schabbach in 1919—a survivor of the western front. He sees the church steeple, pigs on the village lane, and Maria Wiegand observing him. The spiritual aspect of life, farm work, and Maria (or female compassion) are all there as they have always been. He then urinates against the barn to indicate he is back on home terrain and that neither technology (toilets) nor sophistication have any relationship to the eternal verities.

The *Alltag* we witness is one of timeless rhythms of work in the fields, baking bread, gathering berries, and going to church on Sunday. Much is omitted. One is immediately struck by the absence of the market, a place where money and goods are exchanged. Schabbach does not appear to have a shop, nor do we ever see the Simons or the Wiegands selling grain, buying

fertilizer, or negotiating a loan from a moneylender or banker. Instead, there are endless scenes of planting and harvesting in the fields. Unfortunately, buying and selling are as authentic to farm life as plowing, and few farmers do one to the exclusion of the other.

Another omission is the lack of serious conflict in the village. There are practical jokes and some jealousy, but genuine discord is absent until the 1950s. A glance at the villagers' attire makes Reitz's message clear. For example, he shows a stranger being stared at by Wiegand, the mayor and wealthiest farmer, and the poorest laborer, Glassisch Karl. Both wear identical caps, shirts, vests, and pants. Those who leave Schabbach (mainly the males) return with alien clothing and alien accents. Paul Simon returns from two decades in the United States with a cowboy hat and Wilfried Wiegand from Berlin with a crisp new SS uniform. In Schabbach, they look out of place, having been assimilated to alien forces that threaten village life—America and the Nazis.

Heimat is, above all, a film about community and scale. Modernization is the false messiah that has lured Germany away from a vernacular world of traditional values and humane ideals. In *Heimat*, highways result in accidental deaths and farm machines destroy the pure Hunsrück air.

Yet Reitz has no crude Rousseauian view of rural life. Schabbach has a dung pit, and flies are a continual nuisance. Daily chores must be done during rain and freezing temperatures. For the writers, the hard life and simple verities are rewards in and of themselves. They mock the later generations of villagers who are concerned with comfort and appearances. The older Simons never own a car, nor do they have hot water or a toilet in their 200-year-old house. In contrast to the Catholic Simons, the Wiegands are either lapsed Catholics or Protestants and are male dominated. One rarely sees Wiegand's wife, while Katherina Simon seems ever present. The Wiegands are the first to buy a motor-bike in the 1920s, a car in the 1930s, and a milking machine and pesticides after the war. While a picture of Jesus adorns the Simons' kitchen, on the Wiegands' wall we see Paul von Hindenburg, followed by Hitler, and then Ludwig Erhard in the 1960s.

Reitz's picture of Nazis as upwardly mobile Germans concerned with mechanical playthings is rather shallow. The film totally ignores the broad Nazi success in winning artisans and farmers who feared the spread of Bolshevism. In reality, the Hunsrück voted for Hitler over Hindenburg in the 1932 election in contrast to the nation as a whole. The Nazis received their lowest vote in the big cities that Reitz despises.

When the Nazis depart in 1945, black G.I.s chewing bubble gum arrive. Reitz and Steinbach now take on the role of Old Testament prophets, warning their fellow Germans that their souls are threatened by the fool's gold of modernization offered by the Americans. Indeed, the portrayal of the Americans is the shallowest part of the film and quite disturbing. It is difficult to avoid seeing the Americans in the mid-twentieth century as the substitute for the Jews (earlier on) as the rootless cosmopolitans threatening German authenticity and fundamental values. They continue the Nazi obsession with technology and lack a sense of *Heimat*. The caricature of the Jewish nose is replaced by the smile engraved on Paul's face (in a village in which no one smiles) upon his return from America. The Jewish skull cap is symbolically replaced by a cowboy hat. When Paul Simon arrives in New York, a fellow Central European comments on his "innocent and clear German eyes." Twenty years in the United States and he has become a shallow, overweight man who is too busy to attend his mother's funeral. To the tune of an American army band's rendition of Glenn Miller's "In the Mood," he crassly distributes Hershey bars and cigarettes in Schabbach's village hall in 1946.

In *Heimat*, the world of international business and corporate takeovers are American materialism spread to the virgin soil of the fatherland. Not for the first time, we see Germans blaming the ills of the modern world on an external or alien group. The German, with his innocent clear eyes, is being led astray by a foreign serpent. One would have thought that this disingenuous theme had been buried long ago.

Director Reitz appears to be following the path of Rudolf Bahro, a leading East German Marxist, who, upon coming to the West in 1979, began reviving the tradition of *Kulturpessimismus* with Spenglerian jeremiads about the West having gone off the right track in the

Renaissance. For Reitz, who seeks to blend traditional Catholic anti-modernism with a "Green" postmodern hatred for industrial society, the turning point would be the Reformation and the decline of Mariolatry. He is resurrecting a discourse that prevailed in the nineteenth century about the modernization of Germany's society and economy, a discourse that posits stark contrasts or dichotomies, including urban versus rural, male versus female, religion versus politics, Americans versus Germans, academic medicine against homeopathy, and the transient versus the eternal. No compromises or delicate balances are possible.

Because Reitz prefers the traditional to the modern (although he lives in Munich), the past to the present, *Heimat* is a film about loss. At its best, it brilliantly shows the price we have paid for the emergence of the urban welfare states we all take for granted. At its worst, it is a nostalgic, overly sentimental sojourn to a world that Reitz sees as more authentic than our own, a world that is simple but not facile. Max Weber once termed such a fantasy *Harmoniesuppe* (harmony soup).

Kenneth Barkin

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Deutschland, bleiche Mutter [Germany, Pale Mother]. Produced and directed by Helma Sanders-Brahms. 1979; color; 130 minutes. German with English subtitles. Distributor: West Glen Films.

"O Deutschland, bleiche Mutter, wie sitzest du besudelt unter den Völkern."
(O Germany, pale mother, how you sit defiled among the peoples.)

From this heartsick opening line of Bertolt Brecht's 1933 poem comes the motif and title of Helma Sanders-Brahms's film about motherhood and Nazi Germany; the poem itself is read, by Brecht's own daughter, at the beginning of the film: a complex meditation on Hitler's Germany that was also her parents' Germany, on women's origins and identity, on the relationships between men and women, mother and daughter, on depravity, shame, and retribution, on experience, memory, and invention. Sanders-Brahms, born in 1940, uses her identity as literal and metaphorical daughter to construct a fictional biography of her mother, Lene, and of her own individual and national origins. Softly narrated by the voice of daughter Anna/Helma, the film follows the story of her mother's courtship and marriage on the eve of the war, her unsoldierly father's reluctant departure to the war, her own birth, and the struggle for survival by mother and child in the manless world of wartime and postwar Germany. Then, foreshadowed by a wartime sequence in which her father, on furlough from the front, briefly invades the dyad of Lene and Anna, the postwar section of the film shows the disruption of the all-female family by the returning men and the displacement of women from authority and responsibility to helpless marginality. From this point on, the men, whose presence was previously intermittent and shadowy, begin to dominate the film, and the narrative moves to focus on their needs, ambitions, and disappointments. The climax of the film's eviction of women as agents is a histrionic sequence in which a misdiagnosed infection leads to facial paralysis and the wholesale extraction (at the behest of her husband) of Lene's teeth. After this, virtually speechless, she seeks concealment behind an impenetrable black veil that parodies her bridal veil and inverts the film's previously repeated sequences of undressing and disclosure. Finally, with the "normal" family in a crisis of disintegration, Lene locks herself in the bathroom and attempts suicide by gas; her daughter in the film's frame screams for her to come out; and, although we see that she finally does unlock the door, the narrative voice tells us that "Lene is still there, she is still there."

Sanders-Brahms has explained her film as a critical response to Joachim Fest's documen-

tary, *Hitler: A Career* (1977), “which suggested that women surrendered themselves hysterically to Hitler the super-phallus, that they were responsible for what happened in Germany at this time” (Helma Sanders-Brahms, *Deutschland, bleiche Mutter. Film-Erzählung* [Reinbek, 1980], 25). Against this image (previously popularized in Fest’s book *The Face of the Third Reich*), Sanders-Brahms argues that women’s experience actually represented “the positive history of Germany under fascism,” that, unlike the men who fell for a fantasy of heroism, “women’s experience of this was different, more objective [*sachlicher*].” Whether this applies to her film, whether her film is “more objective” than the genre of war films from which Sanders-Brahms also differentiates it, is debatable.

Although Fest’s psycho-reductionism was puerile, trite, and no longer carries historical conviction, its revision here is also less than satisfactory. Such an approach to the history of women under Nazism, which has recently been explored, in different ways, by German feminists such as Gisela Bock, Annemarie Tröger, and Ingrid Strobl, can also be one-sided and exculpatory; a similar charge has been made against the genre of *Alltagsgeschichte*, the history of everyday life, to which this film bears some affiliation. It is disputed in other investigations, notably Claudia Koonz’s well-known *Mothers in the Fatherland* (1987), which paints a much more ambivalent picture of women’s role in Hitler’s Germany, and which, in suggesting how the gender system functioned to sustain Nazi criminality, is also far more subtle than Fest’s simplistic algorithm of women, hysteria, and Hitlerism. Indeed, although Sanders-Brahms wanted her film to represent the honorable story of German women under National Socialism, in some ways it also undercuts her claims, not least by virtue of its complicated narrative strategy. Rather than following every turn of the film as it comments on its own premises, I have chosen to focus on a single significant episode—an episode that Sanders-Brahms herself described as “the crazy monster in the middle of the film.”

The core of the film’s ambivalence is expressed in this long sequence, in which Lene tells her daughter the Grimms’ fairy tale of the bandit bridegroom. This intricate and brutal tale is so crucial in its content and placement that it is worth discussing in some detail. The story tells of a young bride and the suitor to whom her father has given her in marriage. On her way through the forest to visit her suitor’s house, the girl is advised by the birds to “turn back turn back, young bride, you are in a murderer’s house,” but she carries on to the house. When she gets there, an old woman warns her that she is about to “make a marriage with death”: her husband will hack her to pieces, boil, and eat her. Hidden by the old woman, the girl witnesses another young girl undergo this very fate at the hands of her suitor and his gang. The dead girl’s finger, hacked off by one of the murderers as he tries to get her ring, springs from the table into the bride’s own lap as she huddles in her hiding place behind the stove. But, with the aid of the old woman, she is not discovered. She escapes, returns home, and confronts her betrothed at their marriage feast with her experience: not directly but as a dream told to entertain him, a tale punctuated with the repeated phrase “My love, this was only a dream.” As she reaches the point where the murdered girl’s finger fell into her lap, she reveals the severed finger itself to the other guests. “The bandit, who had become chalk-white during the story, leapt up and ran off. But the guests seized him and delivered him to the court. And he and his whole gang were executed for their atrocities.”

This fantasy of horror and retribution is richly layered both in its content and in this film’s retelling of it. Lene begins her story as she and Anna, on their own flight from the war, pick their way into a bombed and ambiguous building, the furnace-house of an abandoned factory, its chimney and ovens inescapably reminiscent. As the child settles to sleep, her mother’s parodic bedtime story tells her “you are in the house of a murderer”; as they leave the factory, they stumble on a decaying corpse, from which Lene shields her daughter even as she continues the gruesome fairy tale, up to the point where the bride is telling her story to the betrothed: “My love, this was only a dream.” The scene cuts abruptly to an airdscape of bombed Berlin—a recurrent, silent image in the film—and then returns to Anna and Lene as two American soldiers come across the pair and rape Lene while Anna watches. “It’s the victor’s right, child,” she says afterwards, “to take things and women.” This episode interrupts the telling of the fairy

tale, which is only brought to its conventionally satisfying conclusion as they continue their journey, squatted—another polyvalent image—between two wagons of a train.

Interestingly, the fairy-tale sequence almost fell victim to the cutting-room floor; its own contest for survival testifies to uncertainties that the director herself otherwise tends to repress. In *From Hitler to Heimat*, Anton Kaes explains this story's links with other allegorical images in the film and rightly probes the troubling relationship of allegory and documentary in a work that seems to conflate them, thereby authorizing its own interpretation of women's blamelessness. We can also pursue this intricate nexus of the film beyond its directorial manipulation of truth and fantasy to the point where the film's own claims begin to seem less solid. Here we have a story within a story within a story—a fairy tale, a fantasy, recounted within the framework of a film whose narrative itself is doubled (a visual account commented on by Anna's external voice). The fairy tale is also told twice, first as event, the second time as, allegedly, a dream, and is interrupted in its telling by the visual images of bombed, "taken-out" Berlin, and Lene "taken" by her rapists: two emblems of war, the one soldered into its specific twentieth-century context, the other conventionally timeless. This narrative confusion and rupture, together with the violent displacement of Lene the narrator to Lene the rape victim (while the child remains virtually silent in both sequences), unsettle the denouement of the fairy tale, reducing it to no more than the *ritual* of closure it represents in the fantasy: a fairy-tale convention repeated in the kiss Anna places on her mother's cheek as she rises from her rape. For the fantasy of retribution may indeed be nothing more than this, a comforting fantasy, a conclusion whose self-evident justice is actually subverted by the economy of the fairy tale, depending as it does on its own premise of violent, repressed desire.

Kaes and others have pointed to the film's equation of Lene's rape with the metaphorical "rape" of Germany by its occupiers, an equation that endows each with the same passive, victim's innocence (*From Hitler to Heimat*, 148). Yet the pleonastic involution of the narrative at this point—produced through at least four different voices—also seems to disclose a moment of intense confusion about the source of truth in evidence and narrative and about the distinction between victim and perpetrator—even as the film faces the essence of Nazi Germany as a "murderer's house" and the ultimate act of patriarchal domination. This confusion is not fully recuperated by the elective symbolism of defiled mother/defiled country, any more than the fantasy of punishment is equivalent to its realization. And the fairy tale's denouement recalls, finally, another stanza of the opening Brecht poem, "Why do the oppressors all honor you, but the oppressed blame you? The exploited point their fingers at you, but the exploiters praise the system that was invented [*ersonnen*] in your house." Brecht's sharp contrast between exploited and exploiters, offered at the opening as an epigraph for women's experience of Nazism, offers a certainty that the film cannot fully sustain. In this sense, it is perhaps not the conflation of documentary and fiction, truth and imagination, that is disturbing in this film but that, at its core, their difference cannot finally be disentangled.

Jane Caplan

Bryn Mawr College

Die Ehe der Maria Braun [The Marriage of Maria Braun]. Directed by Rainer Werner Fassbinder. 1978; color; 120 minutes. Distributor: New Yorker Films. Video: RCA/Columbia Home Video.

This is the first and most famous film in Fassbinder's trilogy on the Federal Republic. (The other two are *Lola*, 1981, and *Veronika Voss*, 1982.) *Maria Braun* is the story of a young woman married in 1944 whose climb up the postwar West German corporate ladder serves as an allegory of what Fassbinder sees as the reconsolidation of social and political authoritarian-

ism in West Germany between 1945 and 1954. The prolific Fassbinder, who died in 1982, was a prominent member of the German New Wave in cinema, directors who by and large were born during the war and radicalized by the youth movements of the 1960s and 1970s. In West Germany, of course, this rebelliousness was aggravated by discontent over their elders' indulgence in the material successes of the "economic miracle," the resultant social and political conservatism of the Federal Republic, and, behind it all, the psychological denial of the crimes of Nazi Germany.

Fassbinder remained a radical utopian whose films nonetheless concentrate on the frustration of hope and change. *Maria Braun* is a prime example of this, since Fassbinder regards the period beginning in 1945 as one in which "everything seemed possible," including the founding of a state "that could have been the most humane and freest ever" (quoted in Anton Kaes, *From Hitler to Heimat* [1989], 79). Instead, democracy was imposed by the Western occupation forces, and the cold war demanded German industry and a German army. *Maria Braun* ends with the death of the protagonist in 1954, the year (mirroring 1945) the West German army was born. Fassbinder underscores this tragedy of blasted hopes, as he sees it, by filling the soundtrack at the end of the film with the last seven minutes of the radio broadcast on July 4, 1954, of West Germany's victory in the world soccer championships: Maria and her husband Hermann die in their flaming villa while the announcer exults: "Time's up! Time's up! Germany is world champion [*Weltmeister*]!"

While Fassbinder's hope for a new beginning in 1945 explicitly accepts the potentially exculpatory notion of a "Year Zero" in German history, *Maria Braun* posits political continuity between the Nazi years and the subsequent history of the Federal Republic. The film opens with a portrait of Hitler blasted away in the bombardment that accompanies Maria and Hermann's wedding and concludes with a series of negatives of the pictures of postwar chancellors Konrad Adenauer, Ludwig Erhard, Kurt Kiesinger, and Helmut Schmidt. (Fassbinder omits Willy Brandt, whom he sees as an exception to blindness to the German past.) And the story ends with the gas explosion that destroys Maria's house, a symbol (applied less skillfully by other filmmakers to America in *Zabriskie Point*, 1969, and *The Two Jakes*, 1990) of the failure of reform and the triumph of bourgeois greed. The film's primary level of narrative, that of the "private history" of the everyday life of common people, implies a tradition of apoliticism. In *Maria Braun*, "public history" regularly intrudes into the lives of the characters, usually in the form of radio speeches and announcements, but the characters remain oblivious to such intrusions. This narrative style, along with the large number of minor characters, allows the film to reproduce effectively the "microhistory" of the period, rendering it a valuable depiction of the early material deprivation and the later material plenty that submerged critical political, social, and historical consciousness.

Fassbinder has argued that women are offered a greater chance for constructive social reform than are men, whose lives are more determined by public opportunity. Thus he casts women as the protagonists of the films in his Federal Republic trilogy. By focusing on what Fassbinder calls the "micropolitics of desire," however, the film abandons inquiry into the responsibility of people like Maria and Hermann, her soldier-husband who remains a captive of the Russians for a time after the war, for action (or inaction) under National Socialism. Fassbinder's depiction of the struggle for survival, especially for women, during and after the war lacks the critical quality of recent historical research, such as that of Annemarie Tröger, which shows that women's accounts of their suffering between 1939 and 1949 have served as a means to avoid confronting their indirect complicity in the greater suffering inflicted by the Nazis on their victims.

This neglect of perspective undercuts Fassbinder's attempt to draw lines of continuity between Nazi Germany and the postwar period. At the same time, Fassbinder's radical insistence on political continuity between the Third Reich and the Federal Republic works against a critical appreciation of the nuances of collaboration and abdication under a dictatorship significantly different in policy and process from West German democracy. We do not learn anything about Maria's (much less Hermann's) life before 1945 that would illuminate the ways in which her survival and prosperity after the war might have been conditioned by her

socialization before *and after* 1933. (Compare Detlev Peukert, *Inside Nazi Germany* [1987].) Maria remains more a symbol of relative innocence corrupted (and opportunity missed) than a fully drawn character. It serves Fassbinder's ideological convictions more than it does history that Maria vomits during an Adenauer radio speech advocating rearmament, that in the end she realizes she has been manipulated by the men in her life, and that her death appears as a possible suicide. In spite of its limitations, however, *The Marriage of Maria Braun* offers significant insight not only into the social history of postwar western Germany but also into the radical critiques of the social and political system of the Federal Republic common during the 1960s and 1970s, critiques powerfully concerned not just with socioeconomic theory and practice but with the substance and consciousness of recent German history as a whole.

Geoffrey Cocks

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Deutschland im Herbst [Germany in Autumn]. Produced by Filmverlag der Autoren; written and directed by Alf Brustellin, Bernhard Sinkel, Rainer Werner Fassbinder, Alexander Kluge, Maximiliane Mainka, Edgar Reitz, Katja Rupé, Hans Peter Cloos, and Volker Schlöndorff. 1978; color; 124 minutes. Distributor: West Glen Films.

On September 5, 1977, Hanns Martin Schleyer, ex-SS officer, chairman of Daimler-Benz, and head of the German industrialists' federation, was kidnapped under bloody circumstances (four companions were killed) by the Red Army Fraction in the latest of a series of dramatic terrorist and counter-terrorist acts. This initiated the so-called German Autumn, which was almost immediately recognized by commentators as a political watershed. In retrospect, it became the moment of the *Wende*, the turning of the public climate to the right after the progressive openings of the Brandt government of 1969–1974 and in anticipation of the return of the Christian Democratic Union (CDU) to government in 1982–1983. But this move toward the right had already begun during the earlier 1970s, with the resolution of the *Ostpolitik* (normalization of the Federal Republic's relations with Poland, the GDR, Czechoslovakia, and the Soviet Union), and the recession of democratic experimentation before a new authoritarian dialectic, in which the growth of left-wing terrorism and a neo-McCarthyite attack on the legitimacy of the left through the *Berufsverbot* (literally, "occupational ban," a system of ideological surveillance establishing criteria of political reliability for public employees) fed each other in a new climate of anger and fear. Moreover, it was crucial that the resurgence of authoritarianism and the strengthening of the state's police powers had occurred under the government of the German Social Democratic party (SPD)—headed by Willy Brandt's dour centrist successor, Helmut Schmidt—for the contraction of reforming imagination was simultaneously a generational polarization, in which the generations that radicalized during the 1960s (and that provided some significant part of the SPD's forward momentum) withdrew their consent.

In other words, the Schleyer kidnapping came at the end of a long political deterioration, in which an older "Germany" seemed to be resurfacing. Extra-parliamentary activists were demonized as mindless extremists ("Chaoten"), while official politics was assailed as fascist in return. The German Autumn brought this process to a head: on October 13, a Lufthansa passenger plane was hijacked to Mogadishu in Somalia in order to force the release of Red Army Fraction leaders imprisoned since 1972; on October 18, a West German commando freed the hostages in Mogadishu, and the same day Andreas Baader, Gudrun Esslin, and Jan-Carl Raspe were found dead in the maximum-security prison at Stammheim in Stuttgart; the next

day, Schleyer's corpse was found in Alsace. This brutal tit-for-tat of terrorist desperation and state violence imparted an element of oppressiveness and menace to West German public life, in which the constitution ("free democratic basic order") was invoked relentlessly against left-wing dissent and the health of democracy tied rhetorically to a strengthening of the state. This turn to the right proved especially alienating for the generation of 1968: after a brief period when New Left energies flowed towards the SPD, the 1970s reopened a sharp cultural cleavage between the SPD and an extra-parliamentary "alternative" milieu.

Deutschland im Herbst captures this mood exactly. A self-conscious collective intervention of leading voices of the new German cinema, produced—unlike the existing achievements of that cinema—independently and without public subsidy, the film sought to break through the public silence surrounding the immediate events, give a voice to fears, and provide a record of the moment. As Fassbinder later said, the point was to keep on talking, to preserve some critical space, to prevent ordinary people from being intimidated into silence. The film's value was to "formulate anxieties." Its most striking feature, in fact, is its movement between the public imagery of authoritarianism and the "private" space of the everyday, in a way that deliberately upsets the latter's safety. With its fragmentary montage of documentary footage, representational reference to Germany's twentieth-century past, and fictionalized vignettes, the film avoids tidy boundaries, erasing the authorship of the particular segments and continually damaging their integrity. Thus its formal structure embodies its main argument, intruding the past into the present, fact into fiction. The film provides no contextualizing information and no depictions of the public political process, whether of government, parliament, and parties or the extra-parliamentary activism of the streets. In place of the public sphere, we are given lonely and individualized images—a figure crossing an empty nighttime street, an encounter with a border guard, a woman's unexpected meeting with a terrorist wanted by the police (also at night), Wolf Biermann singing behind closed warehouse doors, and, most of all, Fassbinder's long segment toward the start of the film, which intercuts a discussion of democracy between himself and his own mother with a series of abusive interactions with his lover, all behind the walls of a darkened, claustrophobic apartment, linked to the outside world by the telephone and radio.

Fassbinder's contribution is remarkable and disturbing. His affirmation of democracy and the paramount need to voice opinions freely in times of crisis, which occurs throughout the scripted argument with his mother, is juxtaposed with the violence and authoritarianism of his personal relations with his lover, in which power and dependency conjoin. (At the start of the segment, he throws his lover out, at the end he breaks down and is consoled.) Denouncing the moral cowardice of restricting civil liberties, he refuses a bed to his lover's acquaintance, turning him out of the apartment against a background of news reports and police sirens. The same point is made by another strong segment, scripted by Heinrich Böll and directed by Volker Schlöndorff (both "terrorist sympathizers," in the press rhetoric of the time), in which a television editorial meeting shelves a production of Sophocles' *Antigone* on the grounds that it might be interpreted as sympathetic to terrorism. Power, the film argues, is being allowed to do its work. Order once again rules in Berlin.

If the personal in the political is one key theme, intimations of the past is another. The film is framed by extended footage of two public funerals—that of Schleyer, which opens the film, and that of the three Red Army Fraction leaders, which brings it to a close. The contrast is powerful: the one an orderly ceremonial of big black cars, *gravitas*, and besuited middle-aged masculinity; the other a large but desultory crowd of young men and women, frequently with covered faces, attended by an army of police surveillance. In the first sequence, the past is silently invoked: former Nazi and CDU chancellor, Kurt Georg Kiesinger, appears on the screen; the scene ends with a scarred face that evokes the dueling fraternities of Germany's authoritarian history. Moreover, a third funeral appears in the film, that of Field Marshal Erwin Rommel, who was forced to commit suicide by Hitler in 1944; the reference cycles back to the present, where we see Rommel's son Manfred, the mayor of Stuttgart, demanding a dignified burial for the Red Army Fraction leaders. Images from the past repeatedly interrupt the contemporary composition—Rosa Luxemburg and Communist marches from the Weimar

Republic, various glimpses of the Third Reich. Versions of *Deutschland, Deutschland über alles*, mostly mournful, sometimes ironic, provide the accompaniment. The montage effect is managed by Alexander Kluge, who not only cut the final film with his editor, Beate Mainke-Jellinghaus, but whose own segment about a disillusioned history teacher's quest for the German past, shovel in hand, is also interjected throughout the film. To an extent, Kluge's contribution provides the connective glue for the overall project. It is his voice, apparently, that provides the occasional commentary. More to the point, it is history, in the form of the intruding imagery of the past and of the history teacher's ironic reappearances, that supplies the film's guiding narrative.

Interestingly, *Deutschland im Herbst* also provided the impetus for a series of major subsequent films, which are continuous with the aims and contents of the respective original segments: Kluge's *The Patriot* (1979), which takes the adventures of his fictional history teacher, Gabi Teichert, further; Fassbinder's "FRG Trilogy" (focusing on "the marriages of our parents"), comprising *The Marriage of Maria Braun* (1979), *Lola* (1981), and *Veronika Voss* (1982); Schlöndorff's *The Tin Drum* (1979); Reitz's *Heimat* (1984); and Sinkel's *Sins of the Fathers* (1986). In this sense, the film lends a certain coherence to the late 1970s–early 1980s moment of the new German cinema, which in retrospect turned out to be its swan song as a relatively unified movement.

Finally, what is the film's relationship to history in the disciplinary sense? At one level, it is a powerful document of its time, and this raises an important general point. The film's minimalist approach to its own context—there is no situating or explanation of the images in the conventional historian's way—is bound to be frustrating for anyone unfamiliar with the Germany of the 1960s and 1970s, and this absence of citations and attributions, or even basic information of time and place, is a typical problem of the filmic medium, including the documentaries produced specifically for historical use. It is unclear how far this inheres in the medium itself, but the visual representations are invariably allowed to do the work of meaning-production by themselves. At another level, *Deutschland im Herbst* constructs an important historical argument by these means, one that marks "history" in the German context as danger. Perhaps this is what film as a self-conscious intervention can mainly achieve from a historian's point of view (it will always have a wide range of documentary uses in the evidentiary sense). It is exceptionally well fitted for capturing questions, or at least forcing them to break cover. It can disturb the controlling narratives of conventional understanding, formulate anxieties, "seize hold of a memory as it flashes up at a moment of danger," as Walter Benjamin put it. It can also affirm. As this bleak catalog of one particularly dangerous moment reaches its end, the terrorists' mourners straggling defiantly through the roadblocks and watching police, a back shot of a brightly clothed mother and young child (her daughter?) exclusively occupies the screen. "Our agony is your triumph," the Joan Baez anthem repeats over the closing credits, and the women's movement, the anti-nuclear and peace actions, and the Greens were already waiting by the set.

Geoff Eley

University of Michigan

Das Schreckliche Mädchen [The Nasty Girl]. Directed by Michael Verhoeven. 1990; color; 94 minutes. Distributor: Miramax.

The Nasty Girl is about one German woman's relentless search for the truth behind her hometown's role in the Third Reich. What starts out as research for a nationwide essay contest sets a young woman (played by Lena Stolze), living in a small provincial city of West Germany, on a journey into the heart of darkness, into the period of German history that is still recent. Beset by attacks from neo-Nazi youths and the disapproval of the city's hypocritical

citizens, the wide-eyed Sonya unearths evidence from the local archives that implicates some of the most respected members of the community—such as a history professor at the local university who was thought to have been a resistance fighter but who turns out to have sent a Jewish businessman to a camp—and destroys the myth of her hometown as a resistance city.

The film begins with a shot of a classical statue and Sonya's voice pondering the stories of past heroes as told in the *Nibelungenlied*. Spurred on by her early success as the winner of a national essay contest, she dips into the more difficult subject of "My Hometown during the Third Reich." Her efforts to divulge the truth about the period meet with resistance at all levels of the community, and she sues the city twice in order to obtain access to the archives. Despite the initial persecutions and contempt from the locals, Sonya is ultimately vindicated by the success of her book. After receiving several honorary degrees, she becomes a heroine and is honored by the city for her efforts.

No, *The Nasty Girl* is really about the rediscovery of ironic humor in German culture that has been lost through the debacle of the Nazi experience. Any attempt to speak about what has so often been called the "unspeakable experience" is a sobering one, yet to find humor in the ironic distance between the past and the present does not necessarily make light of the experience itself. *The Nasty Girl*, with the humor of its background set against the seriousness of the subject, is the most successful and audacious attempt at the illustration of the effects of the Nazi experience on contemporary German culture since Wim Wenders, in *Wings of Desire* (1988), had two ex-angels in Berlin talk on the set of a film that takes place in Nazi Germany.

Michael Verhoeven utilizes the methods of the last great German ironic humorist, Bertolt Brecht, to create a self-referential film that exposes both the folly and courage of people trying to come to terms with a past that is beyond their grasp. The playful childhood of Sonya, filled with moments like toppling over her baby sister's carriage, setting the dinner fish free on the river, and—with a group of precocious schoolgirls—turning a physics lecture into a series of sexual jokes, is contrasted sharply with the grim truth she finds out about her hometown. The lawyer who promises to represent her because he is opposed to any form of racism stares aghast when her brother brings home a date who is black—effectively exposing the hypocrisy of provincial idealism.

Although the theme of the city's complicity with the Third Reich that drives the plot is treated with due seriousness, it is set in the undeniably humorous background of small-town life, with all its old-fashioned morals, pretenses, and anecdotes. And the contrast underlines the problematics of the historical quest.

Actually, *The Nasty Girl* is about the search for a new method of historical representation, especially on subjects thought to be unrepresentable. A roll-up at the beginning informs us that the story is based on the true experiences of Anya Rosmus of Passau and that the film is a mixture of fact and fiction. Yet, as soon as Sonya appears, we become aware that she is making a documentary, or a fictional film based on her real life. This confusion rather than mixture of fact and fiction reveals that the movie itself is not a fiction based on history but rather a history based on fiction based on history.

Hans-Jürgen Syberberg, among other contemporary filmmakers, sought to represent the unrepresentable in films such as *Hitler: A Film from Germany* (1977) through radically hyper-real methods of using puppets, background projections, and overtly in-studio sets, as does Verhoeven, who lets realism go at vital moments of elucidation. In the courtroom scene in which Sonya is suing the city, the slumbering statue of justice in the background stirs and awakens. Most of the indoor scenes are set inside studios with projections on the background. As Sonya's research into the town progresses and her life and family become increasingly exposed to public attention and attacks, the living room where the family is gathered is seen traveling through the streets of the city.

What actually happened in the town during the Third Reich may indeed be unrepresentable and beyond our understanding, as physicists tell us an event cannot be observed without the observer altering it, but, through these carefully considered steps of distancing and hyper-realism, the film attempts at least to get as close to the past event as possible without disturbing the subatomic particles.

Ultimately, *The Nasty Girl* is a parable about the inescapable cooption of truth in modern society. The first part of the film that deals with Sonya's life before the fateful essay contest illustrates numerous instances of corruption in the small bourgeois town, including the system of grading at Sonya's school in which the children of those who made the largest donations to the school were given the highest grades. The meaning and implications of such institutions are not fully revealed to us or realized by Sonya until the dramatic conclusion of the film. As Sonya herself tells us the news of the success of her book and the town's decision to honor her, she smiles and calls it a real happy ending. But the story does not end there. When she unveils a bust of herself at the ceremony, she senses that something is terribly wrong and runs out with her younger daughter, slapping her mother and cursing the dignitaries present in the process. Her own husband at one point confides to the camera that perhaps she is letting herself be used, while the media attention she receives proves to be as frightening for her as the neo-Nazi attacks.

What makes this movie more complex than other recent films that deal with similar topics, such as Costa-Gavras's *The Music Box* (1989) is that it does not end with the self-justifying, simplistic triumph of truth. The question remains, how will that truth now be used, by whom, against whom? At the final ceremony, Sonya senses that she is being honored in an effort to shut her up, which casts a new light on the rapid success of her book as well. She fought to bring to light the dirty truth of the past; could that truth now be used to justify the present that is equally dirty but in a different way?

The film ends with Sonya hiding with her daughter in the tree of mercy where townspeople come to make their wishes and where a gallows once used to stand.

Min Soo Kang

University of California at Los Angeles

The Glasnost Film Festival. Russian with English subtitles.

Distributor: The Video Project, 5332 College Ave., Suite 101, Oakland, Calif. 94618.

The Temple. Directed by Vladimir Dyakonov. Leningrad Studio of Documentary Films. 1987; color; 59 minutes.

Chernobyl: Chronicle of Difficult Weeks. Directed by Vladimir Shevchenko. Ukrainian News and Documentary Film Studio. 1986; color; 54 minutes.

The BAM Zone: Permanent Residents. Directed by Mikhail Pavlov. East Siberian Newsreel Studio. 1987; color; 19 minutes.

And the Past Seems But a Dream. Directed by Sergei Miroshnichenko. Sverdlovsk Newsreel Studio. 1987; color; 67 minutes.

Theatre Square. Directed by Grigor Arutunyan. Armenian Film Studio, Division of Documentaries. 1988; black and white & color; 26 minutes.

This Is How We Live. Directed by Vladimir Oseledchik. Ukrainian News and Documentary Film Studio. 1987; color; 30 minutes.

Homecoming. Directed by Tatyana Chubakova. Moscow Central Documentary Studio. 1987; black and white; 17 minutes.

Marshal Blucher: A Portrait against the Background of an Epoch. Directed by Vladimir Eisner. East Siberian Newsreel Studio, Irkutsk. 1988; black and white; 70 minutes.

The Trial—II. Directed by Igor Belyayev. Moscow Central Television "Ekran" Studio. 1988; black and white & color; 55 minutes.

Adonis XIV. Directed by Bako Sadykov. Tadzhik-film. 1977 [released 1986]; black and white; 9 minutes.

The remarkable student film *Adonis XIV* traces the brief career of a slaughterhouse goat. Decked out with bells and bribed with sugar cubes, the animal obediently leads herds of horses, cattle, and sheep—similarly unquestioning—to their deaths. When, at last, the goat sees the results of its work, it cries out in protest but not in time to save itself from becoming a victim. At the end, its horns are lacquered and mounted alongside those of earlier Adonises. The faceless executioner extends his hand to the audience, making a new offer of bells and sugar cubes.

Soviet censors detected the parallel between *Adonis XIV* and the USSR under Leonid Brezhnev and banned the film in 1977. Nine years later, Mikhail Gorbachev's glasnost enabled it to see the light of day. Glasnost also opened discussion on a host of previously forbidden subjects. Little wonder, then, that the Soviet cinema has taken advantage of the new freedom to explore those long-untouchable topics by means of documentaries, with all of their directness and detail. Even filmmakers known for their dramatic features have turned to the documentary in an attempt to re-create the Soviet past and reexamine the present. The "Glasnost Film Festival" brings together twenty-two of those documentaries packaged as twelve videos, most about an hour in length. With the exception of *Adonis XIV* and another short, all appeared between 1986 and 1988 and represent studios from different parts of the Soviet Union, including several non-Russian republics. The films had their North American debut in 1989 under the aegis of the Citizen Exchange Council and the American-Soviet Film Initiative. Six of the videos, comprising ten films, form the basis for this review. All of them are in Russian with idiomatic, easy-to-read English subtitles.

While many of the "glasnost documentaries" manifest an interest in history, none takes a longer perspective than *The Temple*, a loving examination of Russian Orthodoxy commemorat-

ing the one thousandth anniversary of the conversion of the country to Christianity. Its soundtrack suffused with liturgical chant and church bells, the beautifully photographed film visits some of the most picturesque and important religious centers in Russia, among them the Trinity–St. Sergius Monastery, where in 1389 the forces of Dmitrii Donskoi received a blessing prior to their campaign against the Mongols. Indeed, the connection between Orthodoxy and Russia's history is a principal theme of the film, which points out that nothing of significance occurred before 1917 without the approval of the church. Its difficult experience under Soviet rule is conveyed through newsreel footage of the confiscation and destruction of church property in the years following the revolution. Its patriotic role during World War II also receives attention. Most of *The Temple*, however, tries to demonstrate the continuing vitality of religion in Russian society today. Speaking simply and movingly about their faith are believers such as seventy-eight-year-old Father Nikolai, the monk and renowned icon painter Zenon, and Mother Varvara of the Piukhtitskii Convent, whose agricultural output is the envy of the local collective farm. While acknowledging the indifference of many younger people to religion, the film takes heart from the throngs attending worship services and participating in the restoration of church buildings. It leaves little doubt that the church, now as always, remains a force to be reckoned with. In addition, it suggests the desirability of similar documentaries about the non-Orthodox churches in the Soviet Union, which also have benefited from glasnost.

Soviet history has no more compelling a chapter than the Stalinist era, whose content and legacy have inspired many contemporary filmmakers. Three works from the "Glasnost Film Festival" deal with those turbulent and controversial years. Of them, the most ambitious is *Marshal Blucher: A Portrait against the Background of an Epoch*. It sets itself the task of explaining Stalinism through the story of Vasilii K. Bliukher (1890–1938), one of the most compassionate and popular of Soviet military leaders, who met his end during the Great Terror. Rich in archival footage from the Soviet army and other sources, the documentary moves back and forth between the career of Bliukher, who served with distinction in China and the Soviet Far East, and Stalin's economic and social transformation of the Soviet Union in the 1920s and 1930s. This material is counterpointed with the reminiscences of Bliukher's second wife. Her account of the arrest of her husband and children is heartrending. Although the film fails to offer fresh insights into the causes of Stalinism, it effectively portrays the dilemma of a decent man caught in the gears of a machine that he inadvertently helped to build. Bliukher's advice to his Civil War officers—"Be in no hurry to punish"—stands in direct contrast to the hysterical cries for blood that loyal citizens voiced at the height of the Purge.

The other two works about the Stalin era deal with the attempts of present-day Soviets to come to grips with it. *And the Past Seems But a Dream* follows a cruise ship sailing out of Krasnoyarsk up the Enisei River. Its passengers are bound for a reunion in the Siberian town of Igarka, where, as children of arrestees in the Great Terror, they had been exiled in the 1930s. Blissfully unaware of the real situation back then, the children amused themselves by staging theatrical performances and publishing a newspaper. Now, as adults, they recall a darker side of their past—the miserable living conditions, separation from parents, and sometimes even arrest and penal servitude. The prevailing tone of sadness is a far cry from the angry, accusatory character of *The Trial—II*. Its witnesses to history "have learned to say out loud what has long been kept inside." Among them are participants in the Bolshevik Revolution, who condemn in the sunlight of Red Square the repressions of the 1930s, even as they regret their own acquiescence in them. Nikolai Bukharin's widow reads his last testament, in which he exhorted future leaders of the party to set things right. "I repeated that letter like a prayer," she recalls, "every day in camp." Teachers discuss the need to inform their students about the Purges. Addressing public forums, members of the intelligentsia and army officers advocate a thorough reexamination of Soviet history. When an economist urges that "we must rid ourselves of the fatal heritage of the past or we'll be a people without a future," it becomes clear that this "trial" has turned the tables on the executioners of old and put them in the dock. Yet both of these films, like *Marshal Blucher*, note that the "heirs of Stalin" remain nostalgic for his strong leadership and indignant at his detractors.

The remaining films in the collection focus on more recent history, offering a documentary

look at some of the most pressing problems that continue to confront the Soviet Union. The longest and most informative—*Chernobyl: Chronicle of Difficult Weeks*—deals with the nuclear explosion of April 1986. It was directed by the late Vladimir Shevchenko, whose crew was the first to arrive at the scene of the accident. At its most basic level, the film provides a detailed and fascinating account of the attempts to cope with a disaster of still-undetermined proportions. Shevchenko surveyed the initial damage (with the “voice of radiation” clicking on the soundtrack), interviewed officials at the site, attended emergency meetings of policy makers, and accompanied repair and rescue teams on their around-the-clock missions. *Chronicle* pays tribute to those who helped contain the tragedy, sometimes at the cost of their own lives—the scientists, engineers, army and medical personnel, and volunteers from all over the USSR, many of them women. The experience caused people “to discover in themselves something they had never suspected” and elicited quick results, without the usual paperwork. Shevchenko’s film is especially provocative for criticizing the incompetence that allowed the accident to occur in the first place and then worsen in the absence of an immediate response. His cameras capture remarkable scenes at party meetings where workers accused of abandoning their posts are chastised and dismissed. The film stops short of blaming Gorbachev and the party for delays in publicizing and treating the problem; indeed, the party receives praise for censuring responsible officials in the press. Nonetheless, the evenhandedness and honesty of the documentary ensured it a difficult time with government agencies, which held up its release. In the end, *Chronicle*, with its spectacular aerial views of the ruined reactor, is a sad and sobering reminder that “the atom has two sides,” a warning that all governments ignore at their own risk.

Under Gorbachev, the era of Brezhnev officially became known as the “period of stagnation.” *The BAM Zone: Permanent Residents* tries to justify this label by inspecting one of Brezhnev’s most ambitious projects, the construction of the Baikal-Amur Main Line railroad through central Siberia. It focuses on a single settlement, one of many to which volunteer construction workers, often Komsomol members, flocked in the 1970s. Newsreels portray the optimism and enthusiasm of these early arrivals, their banners proclaiming “We will reap the taiga’s riches!” Instead of riches, the settlers have had to put up with a lack of housing, a denuded landscape, outdoor plumbing, unpaved streets, and even a rundown cemetery. They feel cheated and abandoned. “Siberia is beautiful,” says one resident, “but it needs a real master.” In the film’s view, Brezhnev was not that man. The Soviet leader goes unmentioned, but a volume of his writings on a desk in an abandoned house establishes his culpability. Short but potent, *The BAM Zone* might qualify as a Soviet version of *Roger & Me* (1989), Michael Moore’s popular documentary about General Motors. Its tone, however, is mordant rather than jocular. Only in the final scene does the bleakness give way to resolve, as Soviet voters go to the polls, presumably to throw the rascals out.

Disillusionment is also rife among Soviet youth, according to *This Is How We Live*. The film consists mostly of interviews with several groups of teenagers. Punk rockers and heavy metalists complain about their parents’ phoniness and materialism. Two young fascists rail against “Jewish democracy” and the Communists’ “lousy humanism,” advocating sterilization and the breeding of a race of supermen. A model Komsomol student speaks pessimistically about the chances for perestroika. Commenting on these disparate responses, a sympathetic teacher sees in them the rejection of forced conformity to a stale ideology and empty rituals. If this provocative work is any indication, the Soviet Union has little to hope for from its younger generation.

Finally, two short films deal with developments out of yesterday’s headlines. *Homecoming* is the first Soviet film to interview soldiers who served in Afghanistan. Their somber reflections, enhanced by black-and-white photography, will sound familiar to viewers who recall the Vietnam era in this country. Within the context of traditional Soviet patriotism, however, these sentiments are little short of subversive. One veteran, for example, recalls the excitement of the kill and compares it with hunting a hare. Another remembers the cold reception back home: “All those complacent mugs . . . I felt like smashing everything.” Powerfully shaken by the experience, still another soldier volunteers his services at a home for blind, deaf, and mute children. A bereaved mother declares her pacifism. “Maybe the Afghans have learned the true

value of their country from this war," a veteran concludes, "but what are we to do with our memory of this war?" *Theatre Square* depicts a hunger strike held in the Armenian capital of Erevan in 1988 to protest Azerbaijan's rule over the Armenian-inhabited region of Nagorno-Karabakh. Interspersed throughout are newsreel clips of visits made to Armenia by Nikita Khrushchev and Brezhnev, as well as a Theatre Square memorial service for Stalin, then recently deceased. Politicians may come and go, these scenes suggest, but the will of the people remains constant. During the strike, the people grow in number, their behavior peaceful, their mood confident, as suggested by a banner that reads "Historical justice will triumph!" At the end, however, the riot police march into place, and a breathtaking aerial view frames the massive crowd and a situation awaiting resolution.

Indeed, all of the situations depicted in the "glasnost" films await resolution, as does glasnost itself. That may help to explain the traditional style of these documentaries. While frequently artful, they prefer content to form, because there is so much to tell. In these works, a people seek to reclaim their past, no matter how painful, so as to chart a more secure future. Focusing on various aspects of Russian and Soviet history with unprecedented openness, the "Glasnost Film Festival" at once becomes a historical document as well as a historic event.

James H. Krukones

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Perestroika from Below. Produced by Daniel J. Walkowitz and Barbara Abrash; written and directed by Daniel J. Walkowitz. 1990; color; 52 minutes. Distributor: First Run/Icarus Films, 153 Waverly Place, 6th Floor, New York, N.Y. 10014.

The Soviet coal miner is caught in a contradictory situation. Extolled in Soviet political jargon as the privileged worker earning twice as much as a medical doctor or engineer, he labors under dangerous conditions in antiquated mines and receives less than the basics necessary to sustain life for him and his family because of the lack of available consumer goods. Every day, he risks his life on the job. One miner in *Perestroika from Below* remarks that "when a miner goes down, he says good-bye to life because he never knows if he will come out or not."

In July 1989, a response to this situation came when an estimated 300,000 Soviet miners stopped work and paralyzed 250 mines and factories in five major coal-producing regions of the Soviet Union. Additional actions were organized by railroad workers, who threatened to close critical transportation lines. The much-publicized "perestroika from above" led by Mikhail Gorbachev and his reformers was being threatened with a "perestroika from below" by the Soviet miners. This worker activity echoed an earlier period, the revolutionary crisis that dramatically altered power relations in the country more than seventy years ago.

A startling picture of worker upheaval in the summer of 1989 is captured in a documentary by American labor historian Daniel J. Walkowitz (author of *Worker City, Company Town*, 1978, and co-editor with Michael Frisch of *Working-Class America*, 1983). In the midst of this labor and political turmoil, Walkowitz led a small group of American academics to Donetsk as part of a previously planned oral history project on Soviet miners under a Sister Cities arrangement between Pittsburgh and Donetsk. Arriving at a unique moment in history, the American historians changed the focus of their project to concentrate on the immediate situation, the mass movement that resulted in total work stoppages of coal production in many mines.

A city of 1.2 million inhabitants located some 550 miles south of Moscow, Donetsk in the Donbass coal region has a thriving working-class tradition, complete with migratory labor cycles and periods of industrial crises that date back to the years of industrial development in the 1870s and 1880s. On July 19, 1989, an estimated 4,000 miners walked off their jobs at the

Gorky Mine, one of twenty-one shut down by the workers. The American group arrived after the miners had gone back to work but the strike committee continued to function. While economic demands such as holiday pay and additional monetary compensation for occupational illnesses were of great concern to the miners, they also expressed a desire, shared by miners elsewhere, to reorganize the entire industry and to question who would run the mines and how. This was the first strike of major proportions to challenge Soviet power since the 1920s.

As a labor historian, Walkowitz recognized the importance of documenting these people and their voices. The subsequent interviews reflected these workers' thoughts and ideas, especially how they viewed Gorbachev's program of reforms. Blended with rare, old film footage obtained from Donetsk Television, the interviews and the strike committee meeting become a living history of the Soviet miners' struggle.

The historic film footage is as revealing as the interviews. Images of miners working, of the tools they use and the hardships they suffer, give insight into life among the workers and the state of the coal industry, as well as the propagandistic nature of newsreel footage. At times, the melodramatic background music is reminiscent of the happy, energetic worker of the 1930s portrayed in such films as Leonid Lukov's *A Great Life* (*Bol'shaia zhizn'*, 1940), a romantic treatment of Donetsk miners during the great accelerated strides of Stalin's first Five-Year Plans.

The fetishism and excessive glorification of the worker in the historic footage contrasts sharply with questions raised by miners about production-rate setting in the tradition of the Stakhanovites, or the meaning of communism under Lenin and Stalin, or the role of Stalin in the postwar reconstruction of the Soviet Union. The interviews expose distortions about the triumph of industrialization in the Soviet period and reveal the personal concerns of the miners. The film vividly portrays grievances of the rank-and-file workers, which differ from those announced by current high-level Soviet reformers and past ideologues. For example, one Soviet miner says sardonically, "If you set a record today, it will be a quota tomorrow." Another proclaims, "Our leaders have begun robbing our people instead of helping the working class in building our communism, for which Lenin raised us. They [originally] fought for Soviet power, for the people to live like human beings, to feel themselves human beings."

The strike committee meeting will be of greatest interest to a Western audience, for here is an insider's view of Soviet trade union democracy in action. While fascinating to observe, the confrontational encounter between union officers and strikers becomes clearer if one understands how Soviet industrial relations have developed. Ideologically, the struggle is complex, because a labor dispute in the Soviet Union is not merely a confrontation between workers and management. It also involves a plethora of existing relationships between the party and workers, between the state planning commission's centralized direction and factory administrators, between trade union bureaucracies and workers and the party. In a way only possible with film or videotape, the documentary reveals the strike committee's political inexperience, if not awkwardness, in handling a new situation through debate and democratic procedures. We see the democratically elected strike committee undertaking formidable tasks of dealing with corruption, ensuring adequate food supplies, and countering speculation.

The end of the film shows that the saga of worker confrontation still continues in the Soviet Union. After the first national miners' strike in July 1989, the government made countless promises for improvements that were not fulfilled, and the miners continue to strike sporadically. On March 1, 1991, vast numbers of miners, perhaps half a million, again went out to demand economic betterment and improved working conditions, including greater flexibility in running the mines. Reportedly aligned with Boris Yeltsin, president of the Russian Federated Republic (RSFSR), the striking miners made an additional political demand—the resignation of President Gorbachev and his government. In the process, the miners have formed an Independent Union of Coal Workers.

Perestroika from Below represents a moment in history when workers chose to mobilize in their own interests against forces that stifled their well-being as citizens and working people. The summer of 1989 was a turning point in the process of social change in the USSR. While

national conflicts reached an unprecedented intensity and fierceness, the country at the same time was jolted by workers' strikes, which were stronger and better organized than ever before in Soviet history. As the film clearly demonstrates, Soviet miners were, after years of material deprivation and resignation, taking it upon themselves to do something about their conditions of life and work. The "awakening" has aroused many to relearn basic principles of worker solidarity. It is a new phenomenon for this generation, and no one knows what the final outcome will be.

Anatoli Ilyashov

Los Angeles

NORTH AMERICA

The Civil War. Produced by Ken Burns and Ric Burns; directed by Ken Burns. 1989; color; 11 hours, 20 minutes. Video distributor: PBS Video, 1329 Braddock Place, Arlington, Va. 22314.

What is there to say about a twelve-hour documentary that draws the largest audience in the annals of public television and takes as its subject not body building or rap music but American history? In an age in which the consciousness of visual images has virtually replaced traditional literacy and in which historical ignorance is endemic, Ken Burns's *Civil War* seems like an achievement beyond cavil. It is difficult to find anyone who watched it who was not moved by its images and details, if occasionally overwhelmed by its length. Unless, of course, you talk to American historians, especially those of us trained in precisely the social historical methods on which Burns so successfully draws. Burns used modern historical techniques, at the level of detail and anecdote, to create an accessible, human-scale account of the Civil War. But, when it comes to historical interpretation, to the process by which details coalesce to make events meaningful, *The Civil War* is vintage nineteenth century.

At the level of technique—both filmic and historical—*The Civil War* is very well done and extremely effective. To unify the story across its nine segments and many smaller fragments, Burns has established a series of historical characters, ranging from the Union private Elisha Hunt Rhodes to Mary Chestnut and Frederick Douglass to President Lincoln and General Lee, through whom we experience the war. Their words are read by celebrities, whose familiar voices help us recognize the character each time he or she appears and bridge the distance between the past and the present. Other aspects of the soundtrack, for which Burns was personally responsible, are also notable, particularly the repeated and constant sound of the guns. Like all good television, you can take your eyes from the screen for several minutes at a time and still follow the action. The influence of modern social history has led Burns to tell his story largely from the viewpoint of the common soldier, and the audience cannot help but be impressed with the intelligence, the independence, the experience, and the sheer power of expression that history evokes from such men. Through just such devices of the filmmaker and historian, Burns is able to make the experience of great events imaginable to his audience. Is this not what the social-historical revolution in our discipline has been all about?

The problem with *The Civil War* is the particular meaning the filmmaker chooses to draw from his material, which only becomes apparent in the course of many hours of viewing. To begin with, Burns never acknowledges that a historical event, certainly one of such epochal significance, will of necessity be interpreted, not only in different but in conflicting ways. Contrast this, for instance, to *Berkeley in the Sixties* (1990), in which participants and interpreters

are constantly haggling over their different assessments of the subject. For all its length and magnitude, *The Civil War* contains appallingly few interpretive voices. Even when we are introduced to conflicting perspectives, most notably, those of Shelby Foote and Barbara Fields, the filmmaker obscures rather than highlights the debate between them.

Yet, more than any other event in American history, the Civil War has been and will continue to be subjected to reinterpretation. Our attitudes as a people about our racial differences and about whatever war the United States has most recently fought endlessly reshape what we think about the Civil War. Somewhere between the fourth and fifth episodes of my viewing, the U.S. military began its carpet-bombing of Iraq, and the sounds of Civil War guns that permeate the soundtrack took on new and appalling power for me. Suddenly, I saw the Civil War in terms of a question I had never before asked of it: "What transcendent political goals are worth what amount of human suffering?" *The Civil War* does little to teach its audience this second and equally crucial principle of social history, that historical meaning is found in a constant dialogue between the present and the past.

Not that *The Civil War* lacks an interpretive perspective. Interpretively, *The Civil War* is as much Shelby Foote's movie as it is Ken Burns's. Foote is a charming and witty raconteur, full of amusing Civil War anecdotes well told. His Civil War is the tale of the invasion and defeat of the South, an episode in American history that is ultimately tragic and a tragedy that is overwhelmingly southern. The presence of Barbara Fields, who is extraordinarily composed and effective before the camera, is obviously intended to offset whatever discomfort we might feel at being guided through these events by a white man with a mellifluous southern accent, but the balance between the two is superficial at best. The other Civil War, the Civil War as "glory," as the battle to abolish slavery, gets remarkably short shrift.

Common soldiers are carefully individualized; we learn many names, hear many voices, are told many personal stories. But not a single man or woman who had been enslaved is brought to life in the entire nine episodes. Indeed, Frederick Douglass (whose oratorical powers are strongly evoked by Morgan Freeman's reading) is the only significant black character. The slaves are a vague suffering collectivity, a dusky Greek chorus who suffer under the lash but whose actions, resistance, and individual humanity are largely missing. Without them, *The Civil War* conveys little to the audience about the reality of slavery or helps them to appreciate its demise. As the war nears its end, Burns spends much more time lamenting the fall of Richmond than applauding the 13th Amendment. Someone who did not know the process by which slavery came to an end could not learn it from this account. There is not a word about Reconstruction; we learn more about reunions of Civil War veterans than about the prolonged political battle which the defeat of the South set in motion and through which its consequences for whites and blacks alike were eventually determined.

The Civil War is luxuriant on military detail and very thin on political context. Each major battle is separately portrayed, the character of its generals dissected, and its battlefields, now quiet and reflective, filmed in long, beautiful shots; this is the visual version of the approach taken by generations of Civil War buffs, for whom reenacting battles is a beloved hobby. Missing are the truly decisive *political* battles, which determined what the armies of the North and South brought to each of their physical confrontations. It is never made clear, for instance, that George McClelland's infamous reluctance to take the Army of the Potomac into battle reflected his political sympathies or that the Confederate army was weakened by the periodic, large-scale desertions of soldiers who could not afford slaves to harvest their crops.

For Burns, what seems to determine the outcome of the war is rather something more elusive and universal: "manliness," that combination of battlefield comradeship and idealization of wife and family that turns men into soldiers. "Manliness" is an important if subtle theme of *The Civil War* to which Burns brings great feeling but little understanding. The final episode of the first segment—frequently cited in advertising leaders—is a memorable letter in which a soldier pledges his undying love to his wife and predicts his own battlefield death. The music that plays behind this episode literally haunts the rest of *The Civil War*. Despite many aspects of the film that indict the carnage of war, this aspect of Burns's approach casts a fundamentally romantic character over the conflict and also works ultimately to reconcile North and South. We

are left, for all the evocative details, compelling characters, and battlefield drama, with a healing sense of a common national experience of civil war, rather than of a nation divided over a fundamental issue—racial equality—about which we can still be said to be at war.

Ellen Carol DuBois

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History and Memory (For Akiko and Takashige).
Directed by Rea Tajiri. 1991; black and white & color;
33 minutes. Distributor: Women Make Movies, 225
Lafayette St., New York, N.Y. 10012 (212) 925-0606.

The visual public record offers great events, notable leaders, and mass actions unfolding before the camera, but memory works with a motley brew of personal impressions and imagery poured over those grand moments. Much of the documentary tradition has fought to make visual history part of an acceptable public record, rather than exploring the ambiguous mix of public and private memory. Rea Tajiri's *History and Memory* poses the problem of memory by acknowledging the tangle of popular recollections and patchy information that forms a culture's versions of the past. Videos and films on a similar theme, such as Steve Okazaki's *Days of Waiting* (1989) and Lise Yasui's *Family Gathering* (1988), testify to what *happened* to Japanese Americans during World War II. But Tajiri's half-hour video meditation deals with *how* memory preserves the experience of internment and its aftermath.

Realizing she cannot keep her station tuned, Tajiri decides to analyze the interference in the band. Alongside visual quotes from war propaganda, newsreels, Hollywood spectacles, and the press, she marshals scraps of family memories, photos, 8mm footage, and 1990s home videos of the Salinas Assembly Center and the Poston, Arizona, internment camp. The juxtaposition of these streams of contributing "information" renders the official versions suddenly terribly partial and lends the personal stories an allure predicated more on their status as suppressed memory than on their shocking revelations. Since the everyday life of the families supplied no gripping image tracks, memory must be coaxed from other less certified materials.

The video suggests that forgetting is meaningful on both the public and private sides: social amnesia is as relevant as repression in determining what stories and imagery remain. One sequence wanders through documentary footage of bombs raining on Pearl Harbor, samples scenes from *Dec. 7* (dir. John Ford, 1943), and turns to *From Here to Eternity* (dir. Fred Zinneman, 1953). A soundtrack of a soldier who survived the attack reports that "it sure looks real." Newsreels from Universal and one from Japan note the devastation. James Cagney and a chorus line from *Yankee Doodle Dandy* (dir. Michael Curtiz, 1942) tap a vigorous, patriotic, young American number. Meanwhile, the label "history" flashes on and off over the snippets of archival material and movies. The invocation of these moments of movie glory recognizes that our historical sensations feed willy-nilly on popular imagery.

These dramatically visible bits of public memory collide in the video with sober scenes of Japanese-American families boarding trains with their life's possessions bundled on their shoulders. An image of a young Japanese woman filling a canteen from an outdoor faucet recurs, until we eventually realize that this is the one story that Tajiri remembers her mother telling her about the internment experience. Mrs. Tajiri, in fact, recalls very little about their term in the Arizona camp, except that she "forgot to remember." This modest avowal captures one of the subtle points of the piece: for most ordinary people—even those caught up in large events—their presence is constituted from absence.

In the process of reflecting on how that absence can be turned into a readable visual presence, the videomaker stumbles on the importance of the fragments of memory she can find. Water is a leading thread that connects droplets of personal memory. Her mother filling

a canteen and the sight of a date palm grove grown tall since the inmates planted it in 1942 help orient her search. Frames from an 8mm film of daily life in Camp Topaz supply more wet and dry material: we see young men digging ditches, a solitary girl practicing her ice-skating, a grandmother sweeping snow off the stoop in a blizzard. As the narration says, "when you create a story, you create a picture in your mind. Sometimes the picture returns without the story." The Japanese in California found a dry place and made things grow: the Japanese in Poston, Arizona, found a desert and nursed palm seedlings into a sturdy grove. The identity Tajiri extracts from the private memory she has excavated revolves around a community of cultivators. For the public record, these were internees, victims of racism and wartime xenophobia, but to her they are also gardeners.

"There are things that happen while the camera is watching," comments the narrator. And some do not. One thing no camera recorded was the disappearance of her parents' house. While Mr. Tajiri served in the 442d Regiment and the rest of the family in an internment camp, the government notified them that the navy had condemned their house. When they finally returned to California's coast, it was gone. The hole where the family home once stood is paralleled in the image track by simple text scrolling on black screens. The text describes the view 100 feet above the lawn and sidewalk—the standpoint from which her grandfather's spirit might have been observing the thing that no one actually saw.

How does a viewer look at a visual work that deals with things nobody saw? *History and Memory* often adopts the clashing comparative strategy: place a scene of patriotic gusto next to a sequence showing Japanese-American citizens being herded into the stables of a hastily converted racetrack. Although the popular imagery may not contain the story that those absent from the historical process want to tell, by edging up against this generalized memory, the "absentees" can sharpen their story. Viewers unfamiliar with the history of the Japanese in America should seek hard information elsewhere. But for those who want to examine further the stories they know, *History and Memory* will extend the quest.

For many documentary filmmakers, presenting the past means supplementing the stock of scenes, introducing new heroes, and recording telling comments from the ignored. Tajiri operates on another level: she works over the existing stock of imagery in order to detour its popular meaning from the usual path. Rather than routinely condemning the stereotypes, racism, and clichés that permeate mid-twentieth-century images of Japanese Americans, she pulls them into her own frame of reference. Yes, as the soundtrack comments, "before the war we were ignored, out of focus, . . . the war brought us sharply into view." But, no, those images need not always be seen in the same framework. Much documentary work has concentrated on getting the story straight, while work such as *History and Memory* attempts to see how distortion and story combine to shape our recollections.

Kathleen Hulser

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Berkeley in the Sixties. Produced and directed by Mark Kitchell. 1990; color & black and white; 117 minutes. Distributor: Kitchell Films, 2600 Tenth St., Berkeley, Calif. 94710. Video distributor: California Newsreel, 149 Ninth St., Room 420, San Francisco, Calif. 94103.

This exciting documentary portrays the student movement of the 1960s from the demonstrations of 1960 against the House Un-American Activities Committee (HUAC) through People's Park in 1969. Mark Kitchell uses personal remembrances, film clips, music, and sporadic voice-over narration to describe the decade. The format is surprisingly traditional

given the subject; no cinematic innovations here. Fifteen former Berkeley radicals, each posed in front of larger-than-life photographic panels depicting episodes related to his or her narrative, recall the events in which they participated. The excitement comes mainly from the use of carefully selected television news footage.

For the most part, the popular press has praised the film for its re-creation of events "tracing both the euphoric rise of student activism and the forces that led to its dissolution," as Janet Maslin remarked in *The New York Times* (September 26, 1990) under the heading "Berkeley: Tie-Dye to Just Ties." David Sterritt, writing in *The Christian Science Monitor* (September 19, 1990), commended the film for its "lively and provocative look at protest and dissent, California style." These reviewers found the movie an interesting glimpse into the past, more nostalgia, perhaps, than history, a documentary of a memorable event in times gone by, like the famed depiction of the Washington State bridge that undulated in rhythmic waves before collapsing or the filming of the fiery destruction of the Hindenberg Zeppelin.

On this level, *Berkeley in the Sixties* is a clever and riveting film. Its intelligent use of newsreel footage vividly recalls the heroic struggles, glorious optimism, and disastrous excesses and failures of the Berkeley student radicals. As a historical vision of the decade, it is less successful. The fifteen interviewees (all still socially and politically committed left-of-center) give the impression of the 1960s movement as one dominated by a cadre of leaders like themselves. It is their remembrances that shape the film; their disappointments are those of the movement itself.

This viewpoint leads to an interpretation that blames the excesses of the demonstrators for the failure of the movement. For example, Ruth Rosen, whose leadership role in the 1960s is more a product of the film than a reality, emotively describes her disillusionment over People's Park, where "the worst aspects of the movement emerged." She blames the demonstrators for trying "to provoke the National Guard into fixing their bayonets. They tried to cause skirmishes on the campus." She, however, "found it possible to reject the feeling, the collective hallucination." John Searle, a professor of philosophy at Berkeley, decries the movement's takeover by outsiders, kooks, and crazies whose cynical motives in People's Park created the confrontation. It is the Berkeleyites and outsiders, not the forces of repression, who forced the military confrontation and violent crackdown that marked the end of the decade. Similarly, the film's view of the Black Panthers places the blame for their destruction on their own radical rhetoric and militant posturing.

The film also contributes to a historical myth regarding the origins of the movement. It opens with the 1960 HUAC hearings in San Francisco and immediately jumps to 1963. Omitted are the organizing and demonstrations that dated back at least to 1957 with the creation of Toward an Active Student Community (TASC) and SLATE (so named because it proposed a student election slate and supported a platform for political action). Missing from the narrative are the contributions of students to the Fair Play to Cuba Committee, CORE activities in Berkeley and Oakland, SNCC organizing, and the struggles against capital punishment (Caryl Chessman), campus ROTC, and the speaker ban. Student and community activists participated in civil rights demonstrations, including the picketing of local businesses, and the sit-in for Fair Housing at the state capital in 1963. There is no mention in the film of the journals *Root and Branch* and *Ramparts* that began publishing in 1962 and gave the developing movement a journalistic voice. Even the innovative experimental film society Canyon Cinema helped create a counterculture in Berkeley. San Francisco Bay activists awakened from the slumber of the 1950s to the sounds of the anti-Establishment writings of the Beats. When the Free Speech Movement began its protests, there was already a well-established tradition of dissent at Berkeley, providing spirited leadership for the events that followed. The omission of this background—perhaps because of a sparsity of news footage—leaves unexplained the emergence of the counterculture and an emboldened student body willing to take risks by joining the protests against the university in 1963.

The film's use of personal narrative gives it a sense of immediacy that is at the same time a source of bias, one also found in many of the written works on the subject. Todd Gitlin's book *The Sixties* (1987), for example, describes the decade from his viewpoint as an ex-SDS president

and gives little attention to events in which he was neither a participant nor in control. A similar solipsism affects the writing of James Miller's *Democracy Is in the Streets* (1987). Both of these books are excellent; both written with the fervor of committed radical activists, but both see the 1960s as a period of failed promise caused by the misdirected militancy that developed after 1967. A similar view dominates the film (on which Gitlin served as adviser) and diminishes the participation, spontaneity, and creativity of the ordinary people who effectively opposed racism, poverty, and the war in Vietnam.

There are other problems with the film. Kitchell seems to have added his brief account of the emergence of the women's movement as an afterthought; it is covered superficially at best. One glaring absence from his cast of characters is Mario Savio, who refused to be interviewed. Occasionally, the visual images conflict with the spoken voice. In the beginning of the film, for instance, Archie Brown (unidentified) is shown directing the singing of the *Star-Spangled Banner* at the HUAC hearings, while the narration calls attention to the spontaneous singing by Berkeley students. But these are minor complaints. Other more basic shortcomings stem from the limitations of Kitchell's cinematic vision. The film does not grapple with conflicting interpretations, it presents no new view of the period, and it fails to relate adequately the impact of events at Berkeley to the wider world beyond Sather Gate.

In the beginning of the film, Jack Weinberg, whose arrest at a Free Speech rally was central to everything that followed, says he was looking for meaning in his life. For the moment, he found this in the students' attempts at restructuring the university and the nation, but, by the time of People's Park, he recognized the movement had gone as far as it could go. John Searle recalls that no long-range vision evolved, and he also saw his hopes dashed at People's Park. The 1960s foundered in chaos, amid the collapsed coalitions of blacks, students, and the counterculture. Seen in this way, the film diminishes the impact of that decade on our own time. People-oriented politics, acted out on the streets, transformed American society to a much larger extent than the film recognizes. This, at least in part, is represented by the very success of the women and men who speak to us from the screen, who are still committed to the struggle for a better, more egalitarian America.

Norman S. Cohen

Occidental College

Roger & Me. Produced, directed, and written by Michael Moore. 1989; color & black and white; 91 minutes. Distributor: Warner Brothers. Video distributor: Warner Home Video.

In the winter of 1989–1990, the remarkable box office success of Michael Moore's first film, *Roger & Me*, raised eyebrows across the United States. Never before had a documentary film so completely critical of corporate America received the combination of national publicity and critical honors that Moore's film garnered. That recognition and acclaim were well deserved. *Roger & Me* is a brilliant documentary depiction, and biting satiric dissection, of the social structure and political-cultural dynamics of what is now so often called "post-industrial" America.

Michael Moore's subject is the decline of his hometown, Flint, Michigan, the birthplace and largest production center of the General Motors Corporation. From the mid-1970s to the late 1980s, Flint was rocked by corporate decisions that closed several major factories and moved other important GM product lines out of the city. Together, General Motors' plant closings and modernization programs reduced its Flint-area work force from a peak of 79,000 in the early 1970s to just 49,000 in 1989. The jobs lost were among the highest paying in the country, and they have not been replaced. So the city that Moore recalls in the opening scenes of *Roger & Me*

as a prosperous and happy place to grow up is shown through most of the film as "the unemployment capital of America." This Flint appears on screen as a grim and scary environment with countless blocks of boarded-up homes and businesses and lots of desperate and hopeless people.

Roger & Me blames this man-made disaster on GM and especially Roger Smith, the company's chairman during the 1980s. In the film, Moore constructs a tale of his own carefully staged, futile efforts to get Roger Smith to answer questions about the plant closings in Flint. This technique is very much like what Norman Mailer developed in his "nonfiction novels" of the 1960s: *Armies of the Night*, *Miami and the Siege of Chicago*, and *Of a Fire on the Moon*. Michael Moore is in fact the main character, as well as the narrator, of his own celluloid nonfiction novel. Moore's on-screen confrontations with a variety of receptionists, desk clerks, security guards, public relations personnel, and other low-level corporate "flak-catchers" are among the film's most humorous scenes. But they also effectively disclose the way that corporate decision makers like Roger Smith, who have the power to make or break a community, insulate themselves from the injuries, the pain, and the anger their policies may create.

Moore's pursuit of Smith extends the autobiographical story that opens *Roger & Me*. It also provides the film with a cohesive overall narrative structure and its title. But this story is just one of several that Moore weaves together in this fast-paced, deftly edited film. Deputy Sheriff Fred Ross is repeatedly shown evicting Flint residents, some demoralized and others angry, who have failed to pay their rents. In these depressing scenes, Moore remains behind the camera, directing our eye to the evidence of broken and disturbed lives. At the same time, as the off-screen interviewer, Moore pursues Ross, trying to get him to lower the "it's just another job" shield he uses to distance himself from the human consequences of his work.

Moore's cutting back and forth between his pursuit of Smith and his tracking of Ross culminates in the film's artfully constructed climax. In these final scenes, an ebullient chairman Smith is shown on Christmas Eve at the corporation's lavish holiday celebration giving a closed-circuit address to the world-wide "General Motors' family." Smith's theme is the way that Christmas reminds everyone of the "individual worth of each human being." At the same time, in Flint, Ross and his men are putting an angry black woman and her children, their belongings, and their decorated Christmas tree out onto a cold, cheerless street. The effect of these juxtaposed scenes is extraordinary. By swiftly cutting from one to the other, Moore is able simultaneously to measure the enormous social distance that separates Roger Smith and the working class families in Flint and disclose the political-economic links between them.

Roger & Me achieves a similar revelation with alternating scenes depicting unemployed people trying to make money and scenes of a local elite seemingly untouched by the mass unemployment in their city. Interviews with a man who sells his blood daily and with a woman who raises rabbits in her home, advertising "Pets or Meat" on a crude, handwritten sign, reveal a Darwinian struggle for survival among the unemployed working people of Flint. Moore sets these frightening images against pictures of Flint's upper class at play. A quartet of wealthy women at a country club explain why Flint is still a wonderful place to live. At the annual Great Gatsby party, where the rich sip champagne among unemployed people dressed up as "human statues," a nattily dressed man suggests the poor should just "get up in the morning and do something." The satire here is incisive. We laugh, but we also recognize the pitiful ignorance of the rich who choose not to know the social reality that surrounds them.

The funniest scenes in *Roger & Me* record the futile and frequently absurd efforts of public officials to revive Flint's economy. The humor in these parts of the film stem from Moore's ability to evoke the utter vacuity of contemporary American political culture. Again and again, Moore presents public officials and celebrities trying to solve mass unemployment by either making people "feel good about themselves" or by trying to make downtown Flint over into another "post-industrial" retail/entertainment center. Pat Boone and Anita Bryant are brought to the GM-subsidized Star Theater to raise the morale of the working class. The city itself pays TV-evangelist Robert Schuller to preach a special crusade among the unemployed. "Miss Michigan" parades against a backdrop of decaying stores oblivious to misery around her. The

satiric impact of these scenes is heightened by other parts of *Roger & Me* showing how irrelevant the “pull yourself up by bootstraps” message is in a city where there simply is no work available.

Moore also exposes the absurdity of the city's attempts to attract tourists to a gritty, depressed industrial center. An impressive, but mostly empty, riverfront atrium mall; a huge, bankrupt Hyatt Regency hotel; and the failed \$100 million Autoworld amusement park (where a robot worker is shown singing “Me and My Buddy” to the kind of robot that has eliminated thousands of real jobs) form a collage of absurdities that Moore assembles to condemn the dominant public policy response to Flint's economic troubles.

Roger & Me presents the realities of corporate power and social class in an immediate and compelling way. It also shows how contemporary American politics and policy making have become thoroughly immersed in, and degraded by, the banalities of commercialized popular culture.

Ronald Edsforth

Massachusetts Institute of Technology

Reviews of Books

GENERAL

JANET L. ABU-LUGHOD. *Before European Hegemony: The World System A.D. 1250–1350*. New York: Oxford University Press. 1989. Pp. xvi, 443. \$35.00.

World history is on the rise. Largely driven by the demands of new secondary school curricula, substantial efforts are underway to include the non-Western world in introductory history courses. Most university curricula for history majors and graduate students, however, remain innocent of this sudden trend. How, professors argue, can world history be meaningfully written when it will inevitably include only a smattering of information from each region and period? Historians seeking to answer this question understandably turn toward all-encompassing theories to structure their presentation and choice of material.

Immanuel Wallerstein's "world systems" theory is possibly the most appealing structure for "world historians" to lean on. While incorporating all parts of the world, it explains and justifies Europe's customary historical prominence. But it has one disadvantage. However useful it may be from the fifteenth century on, it has thus far been only suggestive for earlier periods of history.

In attempting to describe a "world system" proper to the period A.D. 1250–1350, Janet Abu-Lughod both criticizes and expands Wallerstein's approach. Critical of his Eurocentrism, she argues that the modern European "world system" is, in large measure, simply the inheritor and continuator of an antecedent "world system" centered on Asia. Yet she emphasizes so strongly the role of Europe in her putative earlier "world system" that she conveys the Eurocentric impression that European participation is, for her, a perhaps unconscious defining characteristic.

Following her theoretical framework and drawing on a wide array of mostly secondary works, Abu-Lughod concentrates on financial and commercial networks within and between the main zones she sees in her "world system": Europe, the Eastern Mediterranean, the Persian Gulf and western Indian Ocean, the eastern Indian Ocean and Southeast Asia, and China. Lacking the type of quantitative data available

to historians of later periods, she adopts a descriptive approach concentrating on urban trade centers.

The result is a useful and stimulating economic history that juxtaposes data from many different regions. As such, the book should prove useful and popular in world history courses. Its broader theoretical ambition, however, falls far short of proof. The period from 1250 to 1350—essentially the period of Mongol dominion from Iran to China—did witness the introduction of European traders to a much wider world, but in most other respects Eurasian economic interconnections were less vibrant than they had been a century or two earlier.

Iran, which is virtually ignored in Abu-Lughod's book, traded with China, India, and Scandinavia before the Mongol onslaught. Spain, which is similarly excluded from consideration, also benefited from earlier commercial contacts with the rest of the Muslim world. The same can be said for North Africa, where the economy had contracted severely by 1200. In short, several parts of Abu-Lughod's "world" were flourishing before 1200, at which time they also had profitable economic relations with regions that, for one reason or another, were no longer so prosperous after 1200.

The aspects of the period 1250–1350 that seem to have caught Abu-Lughod's attention are the extent of Mongol dominion and its support of trans-Asian caravan trading and the beginning of Europe's incorporation into a larger circuit of Eurasian commerce. Of the two, the latter seems to have been the main consideration prompting her to postulate a "world system." Hence, her laudable effort to recognize the commercial vitality, enterprise, and interconnectedness of late medieval Asian traders reads as if Asian trade only became worthy of theoretical attention once Europeans became aware of it.

What Abu-Lughod ends up describing is less a separate "world system" in and of itself than the final stage of a pre-European interregional commercial network that had been substantially more robust in earlier centuries. Although it is plausible that a growing awareness of this network contributed to Europe's later worldwide commercial ambition, Abu-Lughod's analysis of it is too limited in chronological and geographical scope to serve as an adequate descrip-

tion or to make a convincing case that it constituted a "world system."

RICHARD W. BULLIET
Columbia University

HILLEL SCHWARTZ. *Century's End: A Cultural History of the Fin de Siècle from the 990s through the 1990s*. New York: Doubleday. 1990. Pp. 395. \$19.95.

Already well known as a cultural historian of dieting in America, Hillel Schwartz now turns to weightier matters: an inventory of reflections on the ends of centuries since the 990s. Replete with 473 endnotes, the outcome is a learned tome that manages to be simultaneously confusing and charming, unreadable and great fun, almost as if James Joyce had returned to earth and started writing history books.

The narrative thread of the book, which the author loses time after time, only to pick it up again, is a story of expectations. How did people from the tenth century onward perceive the arrival of a new century? Schwartz reviews the discovery by medievalists that the Christian world was not frantic with terror as the year 1000 approached. The late eleventh century featured the First Crusade, and the late twelfth the prophetic writings of Joachim of Fiore, but not until the late thirteenth century did any serious interest in the transition to a new century emerge, illustrated by the first papal jubilee of 1300. Apocalyptic thoughts and events punctuated the 1490s and 1590s but not the 1390s, upstaged much earlier by the ravages of the Black Death. By the seventeenth century, writes Schwartz, Frenchmen at last began to define a *siècle* as a period of one hundred years, and Germans began to make common use of the term *Jahrhundert*.

The 1690s were a relatively uneventful decade, but the 1790s brought with them the French revolution along with ideas of secular progress. For the first time, the century's turn became something to celebrate widely, and writers even looked forward in their imaginations to the turn of later centuries. Journals and societies were founded to honor the new century. Schwartz then romps through the more familiar terrain of the late nineteenth century with its conflicting notions of *fin de siècle* decadence and future technical and scientific marvels and concludes with a pair of chapters on the cold war anxieties and New Age raptures of our own time.

By and large, Schwartz knows whereof he speaks. He cites all of his evidence exhaustively, and although he slips now and then (H. G. Wells published *The Shape of Things to Come* in 1933, not 1935; alas, I was closing on fifty, not forty, when I wrote *Terminal Visions* [1982]), this is not a book teeming with factual errors. Its problem, if it has one, is the author's impish unwillingness to concentrate on his theme and his fondness for interminable lists of miscellaneous oddities. He loves to scamper back and forth through

every decade of every century searching for curious apocalyptic anecdotes, whether relevant or not.

Most of the voluminous medieval material, for one example, is beside the point because, by Schwartz's own account, hardly anyone in medieval Europe gave much thought to centuries or their endings. The discussion of the 1990s is also somewhat askew. The 1990s had not even started when Schwartz laid down his pen. Instead, we are treated to a mélange of stories and comments on everything from the founding of Israel to the already fading fears of World War III. It is really too early to reminisce about our own *fin de siècle*. This book, it follows, is either much too long or much too short. It is too short if the author's true intention was to study visions of human destiny through a thousand years and too long if he meant what he says about exploring perceptions of centuries' ends. But it is also, let us say again, great fun.

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MICHELLE PERROT, editor. *A History of Private Life*. Volume 4, *From the Fires of Revolution to the Great War*. Translated by ARTHUR GOLDHAMMER. Cambridge: Belknap Press of Harvard University Press. 1990. Pp. 713. \$39.95.

The English version of *Histoire de la vie privée* has now appeared in its penultimate volume, covering what principal author and editor Michelle Perrot calls its "golden age." From the first this series seemed to be stretching toward the nineteenth-century apotheosis of private life, when its characteristics and benefits appeared clearly embodied in home and family. Editor Georges Duby in the introduction to the first volume praised the modern institution of a separate sphere, meaning the home, as a "zone of immunity" (p. viii). He warned, however, that the very distinctiveness of the private, on which the greatness of past societies has been constructed, was in grave danger. "Unless we are careful, individual men and women may soon be reduced to little more than numbers in some immense and terrifying data bank" (vol. 1, p. ix). This look at private life in the nineteenth century, then, appears freighted with doing important ideological work.

The story opens in a terrifying moment for aficionados of the private and public dichotomy when, as Lynn Hunt expertly shows, politicians manipulated private life during the French revolution. Although inspired by classical thought, many of them aimed at making the boundaries more permeable between private and public in order to create a perfect social transparency. Moreover, they deployed imagery from the private sphere—that surrounding motherhood, for instance—in the service of rough-and-tumble politics. In the name of proper motherhood, the government did everything from execute the

queen to make it illegal for women to cluster together on the streets or appear at political meetings. (The latter provision maintained its force a century later to permit the government to break up meetings of feminists.) Meanwhile, the apparatus of politics, Hunt argues, became part of household life. Chamberpots, plates, snuff boxes, and articles of clothing were decorated with revolutionary slogans and motifs. Thus, during the revolutionary upheaval the distinction between private and public was in a sorry state, threatened on all sides.

Napoleon rescued the situation and restored fundamental order to France when he articulated the difference once again in his civil code. As part of drawing legal boundaries and carving out a private sphere from the reigning chaos, the code's provisions disadvantaged, indeed impoverished, those who spent most of their time in the private sphere. Married women could not own property, not even their own wages; illegitimate children and their mothers had no claims on fathers' resources. For a woman to conduct any kind of business, she needed her husband's authorization and could not be licensed in her own name. Even the very construction of the marital home rested on governmental fiat: a married woman had to live in the home designated by her husband—and on and on. Governmental reordering of the private sphere, as Perrot's essay "Roles and Characters" explains, gave concomitant sexual, financial, and other legal privilege to the male. On such publicly defined and enforced asymmetry is the romance of a "private" sphere created.

After this chronologically apt opening, the book's logic and organizing principles become less apparent. Catherine Hall's essay on English family life, an effective summary of Leonore Davidoff and her *Family Fortunes* (1987), shows how the private operated among the middle classes. Concern for family intertwined with religious fervor and business ambitions. In a book almost exclusively devoted to France, the English "home" seems the model for domestic comfort. Yet did the English model work in France, or in England for that matter? Hall, like every other author in this volume, portrays a private world of imprecise boundaries, beginning with the fact that magistrates decreed certain of its rules. More problematic for any precise understanding of what the private actually is, Perrot maintains at several points that the family is only the border between the public and private, that is, if historians have come to see family and home as embodying the private, somehow that is thrown into question by this volume. In fact, the subject matter for the book ranges from wedding and childbirth rituals to the conduct of schools, brothels, asylums, and businesses. If Aristotle was able to sketch out a reproductive world based on the need to reproduce the species, his and subsequent labelings of that world as private seem eroded by this "golden age." The concerns of the household filling the dockets of law courts and parliamentary agendas, family members

infecting shops, parks, photographers' studios, beaches, and hospitals, this volume shows that, in spatial terms at least, the private was unbounded, unfocused, and maybe even nonexistent.

Alain Corbin's brilliant exploration of the individual in the last third of the book suggests that we take a smaller unit than the household for an understanding of the private. By the late nineteenth century, the individual was someone composed of microbes, identity papers, and new hygienic habits. He or she was more likely to have a separate bed, keep a diary, and be the subject of therapeutic treatment whether for masturbation, hysteria, or bad posture. Such personal practices, like those earlier called familial and thus private, make the private now seem equivalent to the solitary, what Corbin calls "the failure to connect" (p. 549). Yet individuals always searched out a return to the social, only this time a sociability based on shared solitude. Thus, intimate relations, whether with a confessor, physician, or lover, were prized. Even here, behavior conformed to models, to what language allowed, to what custom, fashion, and the law permitted. If the private had come to signal the solitary and the sexual, even that solitude participated in practices that were socially and publicly generated.

Perrot has suggestively shaped this treatment in unconventional historical terms, giving it a theatrical motif: "The Curtain Rises," "The Actors," "Backstage." Nothing so well as theater captures the ambiguities and many scenes of an alleged private sphere. Seemingly contained, the private, like the theater, looked in many directions—toward the public and the official, toward hidden places or backstage. Its many actors had varied lives, including professional, public, and intimate ones. The private involved props, finances, an array of spaces, and, most of all, indeterminate boundaries (where did theater begin and end?) and an uncertain reality. Although there is much to be said for historical clarity, the illusiveness of this book's narrative path leaves the way open for scholars and students to pursue the many illusions and subliminal ingredients hinted at as part of private life.

Arthur Goldhammer's pleasing translation, its concise sentences and well-chosen words, heightens the volume's sense of interpretive sprawl. Yet the sprawl provides a useful lesson, that is, the very ambivalences of the concept of the private make writing its history a messy affair. Hardly transparent and contained, private life was an imprecise and even inchoate entity, whose supposedly clear, essential nature was undermined by its very historicity. Every attempt at streamlining or taming it with a single theory has only resulted in failure. As the team of authors shows, it has existed in a dichotomous state, shot through with public presences and constraint, looking inward by the nineteenth century through the family and sexuality onto billions of socially framed but solitary selves. The urge to contact the quaint and romantic

or a realm of sweet freedom and individuality may have once inspired social historians to produce reassuring histories of a fully accessible private life. In fact, that seems to have been, according to Duby's message, the original intent of the series. Thanks to Michelle Perrot and her *équipe*, we see the task as something more—more transgressive, more challenging, and hardly finished.

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BRUCE DETWILER. *Nietzsche and the Politics of Aristocratic Radicalism*. Chicago: University of Chicago Press. 1990. Pp. x, 242. \$24.95.

Bruce Detwiler's book is another indication that a political moratorium in Nietzsche studies is lifting. While existentialism held sway, Nietzsche's philosophical activity was severed from his politics, and the relation of Nietzsche's political stance to his thought was side-stepped as neatly as the problem of Nietzscheanism's interaction with Nazism. But the problem of Nietzsche's "politics" grew more insistent as his influence grew in France and America. The French may have clung to a depoliticized Nietzsche, agonizing instead over Martin Heidegger, but in America the question of the relation of German philosophy to German history came to focus as much on Nietzsche as on Heidegger. American political scientists, concerned with integrating philosophers into the history of political theory, led the effort to repoliticize Nietzsche. Detwiler's stimulating study summarizes and clarifies this effort. He does so by gregariously inviting into his pages all those who have sought to repoliticize the image of Nietzsche in the last decade and a half. At each turn, the reader is presented with Nietzsche's views, Detwiler's interpretation of those views, and his critique of rival readings.

Since Detwiler focuses on a series of peaks in Nietzsche's thought, his task becomes the linking of these ideas in a chain. "Aristocratic radicalism" is set alongside Dionysus, the death of God, and the revaluation of all values. An underlying ambivalence, the sense that "Nietzschean politics" is both "intriguing but odious" (p. 5), makes of Detwiler an agile and fleet-footed guide through difficult terrain. A fruitful exegesis follows. Where others avoided the execrable and unsavory, the author seeks them out, worrying his way through Nietzsche's "allusions to supermen, blond beasts, master races, breeding experiments, and also the 'annihilation of millions of failures'" (p. 113). But, although he warns about Nietzsche's "willingness to aestheticize politics in ways that suggest distinct affinities with fascism," his criticism does not become a motor, and his conclusions are vague (p. 113).

Although Detwiler is expansive in his treatment of Nietzsche's most recent interpreters, he closets Nie-

tzsche off from all but the greatest thinkers. The historian may well wonder whether Nietzsche's aristocratic radicalism is really better understood in reference to Plato's *Republic* than to Jakob Burckhardt's cult of Renaissance individualism, Charles-Pierre Baudelaire's avant-garde modernism, and the transcendentalism of Ralph Waldo Emerson's "over-soul." Political scientists have become so involved in a polemic establishing that Nietzsche deserves a place in the history of political theory that, as of yet, they have had little time left over to determine what his place is. By adopting the monumental and the critical perspectives, Detwiler succeeds in delineating a philosophical politics in Nietzsche, but, by ignoring the "timely," he lacks an eye for the contemporary and banal in the "politics" of aristocratic radicalism. The discussion of "Nietzschean individualism" takes on an airy, unreal quality. Moreover, by bringing the programmatic and the prophetic into such sharp relief, Detwiler is forced to rely on a questionable "unity in Nietzsche's thinking" (p. 185), a unity largely derived from the certainties of the young and late Nietzsche. The aphoristic books are ignored until the final chapter, "The Middle Years: Aristocratic Radicalism and the Question of Democracy," where the author defends his disregard of this period. Having little patience with Nietzsche's experimentalism and uninterested in the fragmentary and provisional, Detwiler finds the aphoristic Nietzsche "dispassionate" and sluggishly motivated by a "scientific-contemplative ideal." He concedes that "Nietzsche's political views appear to vacillate" between 1876 and 1882 and that Nietzsche now gave "democracy his qualified endorsement" (p. 171). But he continues to discount the attitudes of this "anomalous" period in which Nietzsche's political utterances are "less interesting (because less original)" (p. 171). Yet there is something decidedly unoriginal in Nietzsche's aristocratic radicalism. The phrase was only used once by Nietzsche and then as a response to George Brandes's designation of him as an "aristocratic radical." It was, he said, "the cleverest thing I have yet read about myself" (p. 189). Part of its "cleverness," no doubt, lay in freeing Nietzsche from his imprisoning isolation by placing him within the then emerging modernist avant-garde. To Brandes, the programs and prophecies were all conceits, and he was drawn to Nietzsche as a scout of a new aesthetic future. To Nietzsche, Brandes was the one "free spirit" he had been looking for ever since the middle years. At the end of a century in which Nietzsche's apocalyptic vision has proved to be all too timely, it might be valuable to reconsider Nietzsche's aristocratic radicalism as the very thought that linked him most to his contemporaries and their illusions.

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PAUL ROAZEN. *Encountering Freud: The Politics and Histories of Psychoanalysis*. New Brunswick, N.J.: Transaction. 1990. Pp. xxi, 348. \$34.95.

Had Paul Roazen stated in his introduction that he was assembling his many book reviews under chapter headings, I might not have minded my sense of déjà-lu of his controversy with Kurt Eissler on Freud's insensitivity to Victor Tausk's wish to be analyzed by him rather than by Helene Deutsch (pp. 96–117) or Roazen's attack on Peter Gay's *Freud for Historians* for failing "to distinguish between psychoanalysis as an intellectual entity and as a trade union" (p. 263). Nor would I have been so taken aback at his total omission of Gay's biography of Freud (1988), of prominent psychohistorians such as Peter Loewenberg, Robert J. Lifton, and Carl Schorske; and of such psychoanalytic sociologists as Nancy Chodorow, Robert Endleman, Jessica Benjamin, and myself. I would have realized that this compendium of (mostly verbatim) encounters was determined by assorted editors rather than by the author himself.

Two chapter titles flag Roazen's overall bias, his simple division of psychoanalysts into bad guy "loyalists," "New York City's Kremlin-like orthodox psychoanalytic society" (pp. x, 185), which includes "hagiographers" such as Eissler and Ernest Jones, and good guy "heretics," who include such "dissidents" or "excommunicants" as Carl Jung, Alfred Adler, Otto Rank, Wilhelm Reich. (He ignores the fact that Gay also is critical of Eissler for restricting access to the Freud archives and draws no conclusions from the fact that the Jung archives are sealed.)

Roazen's frame of reference is "American" psychoanalysis (including Canada). Thus, his otherwise fair reviews of Freud's case of "The Wolf-Man"—by Patrick Mahony (1984), Muriel Gardiner (1971), and Karin Obholzer (1982)—ignore Nicolas Abraham and Maria Torok's *Wolf-Man's Magic Word* (1976; 1986). They argue that because the Wolf-Man's native language was Russian, his analysis with Freud (in German) must have been somewhat inaccurate. This is a crucial point in a book published in 1990. Roazen's introduction also promises more coverage on France than the superficial treatment of Sherry Turkle's *Psychoanalytic Politics* (1978), François Roustang's *Psychoanalysis Never Lets Go* (1983), and Celia "Bertin's well-paced narrative concentrat[ing] on Marie's [Princess Bonaparte's] colorful private life" (pp. 194–95). (Throughout, women psychoanalysts are called by their first names, men by their surnames.) Roazen's unfamiliarity with psychoanalysis on the Continent is even more blatant. According to Roazen, Geoffrey Cocks, in *Psychotherapy in the Third Reich* (1985), is not critical enough of the Nazi therapists, is "morally out to lunch" (p. 35). Roazen concludes that, if Cocks were correct, "Germany as a country and culture would stand indicted anew" (p. 35). He thereby ignores the fact that German "psy-

choanalytic heretics" are doing just that in, for instance, their journal *Psyche*.

In defense of Roazen I must add that the breezy writing, snide remarks, and repetitive clichés such as "inside dope," "hanky-panky," and "howlers," which tend to grate, are acceptable in journalistic reviewing for *The Nation*, *The Chicago Sun Times*, or *The Saturday Review*. In a scholarly book we expect more substantive and structural analyses than in media publications where, for instance, it may not matter that Roazen considers Erik Erikson "the most eminent contemporary proponent of ego psychology" (p. 152). But in both "loyalist" and "heretic" psychoanalytic circles, this term designates the theories of Heinz Hartmann, Ernst Kris, and Rudolf Lowenstein. Is it possible that Roazen does not know this, or does he think we will not notice his "howlers"? Or has he concluded that what he claims is his own "originality on the subject" (p. ix) transcends factual questions about the authors whom he has reviewed over the past twenty years?

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GARY GUTTING. *Michel Foucault's Archaeology of Scientific Reason*. (Modern European Philosophy.) New York: Cambridge University Press. 1989. Pp. xiii, 306. Cloth \$39.50, paper \$12.95.

It is ironic that Michel Foucault's work, which began by elaborating a concept of the understanding of the human sciences as an archaeology, should itself now be buried by deposits of historical exegesis of a complexity recalling the site of Troy itself. Gary Gutting's book is aimed partly at those stumbling for the first time over this much-quarried site. Concentrating on Foucault's early works, *Folie et déraison* (1961), *Naissance de la clinique* (1963), *Les Mots et les choses* (1966), *L'Archéologie du savoir* (1969), and *Surveiller et punir* (1975), Gutting aims both to give a comprehensive summary of each work and to offer a new perspective on Foucault for more experienced interpreters by placing his thought in the context of recent French history and philosophy of science, by which he means the ideas associated with Gaston Bachelard and Georges Canguilhem. A subsidiary aim of the book is that it should also serve as an introduction to the thought of Bachelard and Canguilhem themselves.

This is a complex, many-layered project, and it is to be questioned if all of Gutting's purposes can be contained within the covers of a single volume. Although his summaries of individual works are both fair and thorough, Gutting's attempts to compress the complex work of two other major thinkers into manageable shape inevitably results in considerable oversimplification. A more serious problem, however, arises from the fact that Foucault's work can-

not be fully understood in the context of any two given thinkers. This discrepancy between the level of a broad general synthetic summary of Foucault's work and a very specific argument about its relationship to a small fraction of the available context of Foucault's development sometimes leads to a distortion in the structure of Gutting's argument. This is particularly evident when Gutting attempts to defend Foucault from the often-repeated charge that his claim for "archaeology" as a way of putting a critical reason to work on the world is in fact fraudulent, because it provides us with no way of recognizing what would be a better world. To counter this charge, Gutting has to abandon his declared aims of concentrating on the earlier works, on Canguilhem, and on Bachelard, and switch forward to some of the last major works that Foucault wrote, among them, his well-known debate with Jürgen Habermas on the meaning of the Enlightenment, in which Foucault attempted to appropriate and develop Kant's critical reason in the service of what Foucault described as "liberation."

Gutting's attempts to link Foucault to the projects of Bachelard and Canguilhem also does him disservice in another way. Gutting tries to defend Foucault's archaeological concept of knowledge from Habermas and Charles Taylor, who with many others have argued that Foucault's link of knowledge production with the structure of power means that Foucault's own thesis must be part of that very structure of power. Gutting attempts to counter with an argument that Foucault absorbed Bachelard's and Canguilhem's idea that the natural sciences such as chemistry and physics did contain a residue of "objectivity," unlike the human sciences, and that it can therefore be said that Foucault did not believe that all knowledge, including his own, was so shaped by its relationship to power as to be unable to generate a critical use of reason. It is unfortunate that Gutting does not discuss how it was that Foucault came to rely so much on a characterization of science that has long been problematic to historians working within an Anglo-American framework. In spite of these problems, however, Gutting's account, though ultimately strongly supportive of Foucault, is also reflective and clearly written and could certainly be offered as a synthetic account of the early works to a nonexpert audience.

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NORMAN J. G. POUNDS. *Hearth and Home: A History of Material Culture*. Bloomington: Indiana University Press. 1989. Pp. x, 437. \$57.50.

After completing his massive three-volume *Historical Geography of Europe* (1973–85), Norman J. G. Pounds set to work on the present study, which covers the

same vast scope of his trilogy, Europe from the Iron Age to the eve of the twentieth century, and draws together his compendious knowledge of the physical and material world.

The book is divided into three parts. The first offers a brief survey of non-European origins and Iron Age and classical expressions of material life. The third is a similarly brief and selective appreciation of the ever-accelerating technical and organizational change characteristic of Western society since the Industrial Revolution. The heart of the book is the lengthy second part, which surveys in seven chapters the material culture of Europe from the fall of Rome to the Industrial Revolution. In Pounds's discussions of village and field patterns, agriculture, domestic architecture and furnishing, demography, urban life, and industrial technology, he calls attention to developments over time and variation across space, but his emphasis is placed on the enduring patterns, the essential sameness, of European material life. Pounds shows a keen eye for the details that support the thesis of continuity in everyday life.

There is much to savor in this wide-ranging book. Pounds expounds on the development of medieval latrines, the shape of keys, the diffusion of gas street lanterns, the Ottoman origins of smallpox inoculations as a precursor to vaccination, and hundreds of other topics. Some will be familiar to the student of European economic life; his discussions of medieval technology and preindustrial demographic processes add little to the many previous works on these subjects. But on many other topics, particularly those concerning domestic architecture and metallurgy, Pounds's wide reading and technical knowledge inform fresh and insightful discussions.

The book satisfies less as its author moves from the concrete to the general and from generalization to analysis. Pounds is quick to generalize, as one must be when working with such a vast canvas. But the generalizations tend toward the bland; they sidestep challenging problems with compromises and confident assertions of commonplaces. Moreover, the generalizations are sometimes wrong. It simply is untrue that "most people before the nineteenth century never handled a coin of precious metal" (p. 313) or that in the preindustrial era "few people ever left the parish . . . in which they had been born" (p. 350).

Historians today have a keen interest in everyday life and material culture; Fernand Braudel's three-volume *Capitalism and Material Life* (1973), Eric Jones's *European Miracle* (1981), and a large number of monographs based on probate inventories will serve to recall the variety of approaches that have been developed to address these new concerns. Pounds's book lacks the vision of Braudel and the comparative perspective of Jones. And it does not dwell for long on the challenging problems of explaining the emergence of a consumer society or the early attentiveness to technological improvement. Instead, Pounds offers an amazing fund of knowledge

knit together with an eclectic mix of anthropological, ethnographic, and geographical concepts.

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JEANETTE GREENFIELD. *The Return of Cultural Treasures*. New York: Cambridge University Press. 1989. Pp. xviii, 361. \$44.50.

In 1971 the Danish government handed back to Iceland the manuscript copies of *Flateyjarbok* and the *Codex Regius*, thereby initiating the return of a collection of manuscripts that Iceland claimed was the key to their cultural identity. With the detailed history of this episode, Jeanette Greenfield opens her study of states attempting to gain back purloined patrimony from foreign cultural institutions. Obviously, this runs counter to museums' *raison d'être*, which is to collect, preserve, and not give away. Greenfield relates not only the long and tangled negotiations for the return of artifacts in many obscure cases, including Tasmanian skulls to Australia, Maori Taranaki panels to New Zealand, manuscripts and pictures from the Caves of the Thousand Buddhas to China, North American Indian artifacts (the Speyer collection) to Canada, skulls to Zambia and Kenya, the Stone of Scone to Scotland, the Ranjit Singh throne to India, the Sphinx's beard to Egypt, and numerous collections of artifacts to various Central and South American countries, but also more famous cases, including the Elgin Marbles requested by Greece and the Benin bronzes requested by Nigeria.

A brief account of how these objects and others were removed from their places of origin and found their way into museums and private collections in Western Europe and North America is only part of Greenfield's study. Greenfield is more concerned with the legal and ethical justifications for the return of cultural property and the byzantine array of legal and other arguments raised by museums and governments resisting return. Beginning in the 1950s under the auspices of the United Nations Educational, Scientific, and Cultural Organization (UNESCO), the concept of restitution of cultural property began to take shape and focused on "the removal of such property as the result of the previous colonization of newly independent states, and the issue of the continued illicit worldwide traffic in art treasures" (p. 214). Through a series of "Recommendations," but more specifically through the Convention on "the Means of Prohibiting and Preventing the Illicit Import, Export and Transfer of Ownership of Cultural Property" in 1970, an attempt was made to define cultural property and gain international agreement on "cultural protection and return." The term "cultural property," however, proved vague and allowed for broad interpretation. This prompted some nations, such as the United Kingdom, to reject the

agreement. Despite the lack of precision in wording, by 1987 sixty nations, including the United States, had become signatories.

Because this book focuses primarily on the legal issues and judicial rulings of various countries opposed to the return of cultural property and to UNESCO's recommendations, it is of limited use to historians except perhaps to those in art history and international law. It is more valuable to museum personnel, however, as it indicates the scope and direction of the international concern for the restitution of cultural patrimony.

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SUSAN SHEETS-PYENSON. *Cathedrals of Science: The Development of Colonial Natural History Museums during the Late Nineteenth Century*. Buffalo: McGill-Queen's University Press. 1988. Pp. xii, 144.

W. A. WAISER. *The Field Naturalist: John Macoun, the Geological Survey, and Natural Science*. Buffalo: University of Toronto Press. 1989. Pp. x, 253. \$30.00.

Historians of science have, in recent years, investigated the mechanisms by which imperial policies and colonial expectations have worked together in the establishment of institutions that furthered their intellectual, social, and economic goals. British imperial authorities, in particular, encouraged geological surveys, natural history museums, and botanical gardens that became, in turn, part of an exchange network for data, specimens, publications, and trained personnel. As W. A. Waiser and Susan Sheets-Pyenson demonstrate, however, the activities initiated by naturalists in the hinterlands and the priorities of local politicians together determined outcomes that were often quite distinct from those envisioned in the homeland.

John Macoun, according to Waiser, was a Canadian immigrant from Ulster whose extraordinary efforts as a field naturalist documented Canadian natural history and reinforced aspirations for the agricultural development of western Canada. Macoun began life as a farmer, turned to schoolteaching, studied local plants as an interested amateur, and was eventually invited to participate in several summer railway surveys in the 1870s. His enthusiasm for the agricultural possibilities of the Northwestern Territory certainly contributed to his appointment as botanist for the Geological and Natural History Survey of Canada in 1881. He worked initially under A. R. C. Selwyn, a geologist educated in England, trained on the British survey, and seasoned by several years on the geological survey of Victoria, Australia. Macoun, however, found it necessary to send unfamiliar botanical and zoological specimens abroad for identification. He repaid colleagues such as C. Hart Merriam at the U.S. Department of Agriculture by providing duplicates for their institutions. Lack of reference materials and

training sustained an intellectual dependency even as local legislative demands drove a practical line of investigation.

Thus, considerable attention was given to mapping westward routes for migrants and to documenting the climate as well as the agricultural and mineralogical potential of largely unexplored regions of Canada. Macoun, with the enthusiasm of an immigrant and an awareness of the political environment in Toronto, typically emphasized the most optimistic theories regarding isothermal lines and growing conditions. His positive assessment of the Peace River region in present day Alberta, for example, provided through informal reports and public lectures, helped insure his steady employment during years of considerable political turmoil. By contrast, his son James Macoun's more "realistic" assessment of the region in a report in 1904 nearly cost him his job and constrained his expedition work for five years. Wasser's detailed and documented account is unflinching in its demonstration of the difficulties of doing scientific research in the Canadian colonial setting.

Another, more comprehensive, account of the natural history enterprise is available in the comparative study of colonial museum development by Sheets-Pyenson. Here, too, the emphasis is on the ways in which local museum directors, often educated in Europe or Britain, mobilized local resources. The basic schema for museums was imported. Whereas Waiser concentrates on the taxonomic enterprise that provided data for both scientific and economic interpretation, Sheets-Pyenson elaborates on the importance of public display in massive "cathedrals of science."

Local specimens were fundamental in colonial museums. They were the basis for the assessment of regional resources as well as the currency for international exchange. Colonial museum directors therefore gathered whatever they could, emphasizing rare and exotic specimens, in order to educate the local populace and contribute to the international network of scientific institutions with which they hoped to become identified. While the latter brought prestige, an account of local natural history was essential for survival. What interests Sheets-Pyenson most is the enterprise that local directors brought to their task and the independence they managed to establish.

Sheets-Pyenson's account of museum building in Australia, New Zealand, Canada, and Argentina begins with the European imprint on these institutions, not least through the influence of imported administrators. Frederick McCoy, for example, emigrated from Britain to a professorship at the University of Melbourne and, shortly thereafter, a position as head of the National Museum of Victoria. His flair for the dramatic made him and the museum a place of considerable public activity. Moreover, his discussion of the mineral deposits of Victoria and the demonstrations of mining apparatus acquainted the citizens of southeastern Australia with possibilities in its still-

unexplored frontiers. Like other successful curators, he learned to accommodate local legislators, prominent citizens, and fellow naturalists as he tailored an institution to fit national needs.

Together, these case studies of natural history at the turn of the century provide a view of science in settler colonies where British influence persisted but where local institutions rather quickly took their own direction. They challenge any simple explanation that portrays imperialism as exploitative or suggests that the dependence in science was unidirectional. The directors of surveys and museums were quite aware that their data were important for the larger scientific enterprise and managed to find a role that permitted them significant participation in the ever-expanding international network of scientists.

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PETER STEAD. *Film and the Working Class: The Feature Film in British and American Society*. (Cinema and Society.) New York: Routledge. 1989. Pp. xi, 283. \$47.50.

One of the central paradoxes of American and British film history is that the movies, which were born among the working class and prospered for decades by appealing to a mass audience, have virtually ignored the world of work. As Dwight Macdonald once observed, the topic of work for the movies was "the dark side of the moon." Moreover, the hope of critics such as Walter Benjamin that film might be a revolutionary vehicle was disastrously undercut. As Peter Stead puts it in his useful book, "to historians of the left the new popular culture represented a fatal and quite decisive fragmentation of what should have been a working-class consciousness" (p. 3).

Stead does not lament the failure to develop a workers' cinema. He argues that "the great triumph of film and especially the American film was that it . . . successfully negotiated the mine-field of class" (p. 239). The film companies maneuvered to overcome their working-class origins and gain middle-class attention and respectability—without losing their mass audience. Central to this process, which reached its apogee in Hollywood in the 1930s and during World War II, was audiences' identification with the star system and the values it promoted, in particular American individualism. The author endorses producer Walter Wanger's claim of 1939: "The American screenplay presents a perpetual epic of the ordinary unregimented individual" (p. 98). Stead's argument needs to be weighed seriously by historians who wish to understand the movies' appeal to the working class.

With the collapse of the studio system and the development of more specialized audiences, working-class subjects have perhaps achieved more vitality, as

suggested by Stead's discussions of American products such as *The Deer Hunter* (1978) and *Norma Rae* (1979) and the British triumph by Hanif Kureishi, *My Beautiful Laundrette* (1986). The convergence of the issues of work, race, empire, and sexual orientation in the latter—unthinkable when Hollywood made films for a mass audience—is surely suggestive about the relationship between the sociology of filmmaking and film content.

The connection linking the politics, economics, and sociology of filmmaking and film content is the book's weakness. Stead slights the attempts studied by Steven Ross to establish a genuine workers' cinema before the studio system's triumph and skirts the impact of political and economic power in determining the treatment of worker-related subjects. He offers minimal analysis of working-class subjects in silent films. Eager to praise the gritty realism of Hollywood's occasional working-class pictures, Stead ignores their political messages. He overlooks the mining industry pressure that caused Warner Brothers to improve mining conditions in *Black Fury* (1935) and soften the prouction message. Fascinated with the scenes of steelmaking in King Vidor's *An American Romance* (1944), Stead minimizes the celebration of the captains of industry and the equivocal portrayal of cooperation between management and labor. He avoids the political fury over *On the Waterfront* (1954). In a book about film and the working class, Stead offers little evidence of working-class responses to film, relying instead on contemporary elite and middle-class reviews. His chapter on British films substantiates the judgment that they conveyed "the faintest dribble of real English life" (p. 111).

The strength of Stead's book lies in his able recreation of the cultural milieu of early movies, a thorough survey of critical responses to working-class issues in film (especially on the left), and the ambitious effort to compare American and British feature movies (in which Hollywood easily scores the highest). Historians now need to examine more fully the political and economic forces that foreclosed alternatives to the studio system's feature films and constrained the movies with internal and external censorship. By analyzing both the contextual and aesthetic forces at work, we may finally understand why the movies have been simultaneously popular and thematically narrowly confined.

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BERNARD MCGRANE, *Beyond Anthropology: Society and the Other*. New York: Columbia University Press. 1989. Pp. x, 150. \$28.00.

It is hard to know whether anthropology is a discipline in full decline or whether it is merely experiencing the writhings of a paradigmatic shift. In any case there has come to exist in the field a whole genre

that I can only think of as anthropology-in-agony. Book after book appears in which the epistemological, moral, and political foundations of the discipline are called into question. The book under review belongs to the genre, and its bibliography lists some of the more important recent works in the same vein.

This short (129-page) essay surveys the history of European perceptions or "constructions" of "the Other" from the Renaissance to the nineteenth century. Bernard McGrane argues that Westerners failed throughout this entire era to perceive the Other in any autonomous reality, thanks to blinders established by the changing epistemic frameworks of Western thought. During the Renaissance the West encountered non-Europeans chiefly as anti-Christians, heathen degenerates fallen from a state of grace. The Enlightenment conceived the Other in terms of a bipolarity of knowledge and ignorance. The nineteenth century saw the emergence of geologic time, which made possible biological evolutionism and with it the view that the Other was not a genuinely different being but merely the modern European in a prior stage of development. The conclusion draws the survey up to modern anthropology, which has been dominated since the early twentieth century by the concept of "culture." The modern paradigm consigns all alterity to a plane of "cultural difference" that now purportedly derives from dialogical, or at least participatory, anthropological activity but that in fact continues to establish the anthropologist as the observer who speaks and the Other as the silent subject. In short, anthropology is "analytic monologue or masturbation" (p. 125).

McGrane does not offer any escape route for anthropology and indeed seems to suggest that the field workers who continue to go forth are doing irreparable damage of the sort that anthropologists themselves used to blame on capitalists and missionaries. He also seems to see Carlos Castaneda as exemplary for recognizing that if the anthropologist truly seeks "membership in the alien culture"—the only procedure that can overcome the reductive, invasive quality of anthropological inquiry—he or she must cease being an anthropologist. The book stops short of recommending that anthropologists close shop altogether, although that seems the only conclusion to draw from an essay that denies that knowledge is cumulative and that inquiry leads to anything but self-contemplation.

As a history the book has some fine moments. The author ably characterizes decisive transitions in the Western intellectual tradition such as the revolution in cosmography that created the concepts of "ocean" and "continent" as well as more minor themes such as the Enlightenment's assimilation of exotic pagans to those of antiquity. McGrane's determination to fit the thought of each era into one framework, however, often leads him astray. Discussing the nineteenth century, for example, McGrane argues that anthropologists of that era conceived the Other exclusively

as child to the European man. This is to write E. B. Tylor large: not all nineteenth-century anthropologists conceived difference as created by "history" rather than by "nature." This representation of nineteenth-century anthropology fits McGrane's critical theme that anthropology has been, and continues to be, just a way of talking about oneself. But it is historically false and obscures, among other essential items, the whole thematic of biologically ordained, unalterable race difference.

This book was written, as its author states, under the "haunting specter" (p. 1) of Michel Foucault. It certainly mimics the Foucauldian style with its endless rhetorical questions and purplish prose. Of the prose, one example may suffice: "In the nineteenth century a strange new formation began to become visible on the outermost distances of the European horizon. At first it appeared but a small rise on the horizon, as a wave, that, instead of cresting and breaking as usual . . . began rather to slowly ascend and grow, to rise up more and more, higher and higher, and disturbingly to form itself on the hitherto flat horizon of the Enlightenment, until . . . one day it finally towered up over the European horizon lifting that very horizon itself up, up and back over itself like a vast and gigantic tidal wave, and in the midst of this loss of horizon, in the center of this sheer cultural vertigo, there occurred the sudden, epistemologically violent eruption of time into space" (p. 89). Others may find this attempt at continental style impressive. To me it ends by sounding like caricature. The assumption of the grand style is all the more irksome in this book because it is combined with gross editorial negligence. There are misspelled words on virtually every page. Proper names are botched ("de las Casa," "Isadore of Seville" [pp. 8, 10]). The word "principle" is used as an adjective throughout the book. This disdain for precision in little things does not improve the impact of a book that in other ways, too, seems careless and over-inflated.

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NICHOLAS THOMAS. *Out of Time: History and Evolution in Anthropological Discourse*. (Cambridge Studies in Social Anthropology, number 67.) New York: Cambridge University Press. 1989. Pp. x, 153. \$37.50.

In this stimulating book-length essay, Nicholas Thomas argues that the privileging of ethnography by anthropologists as method, text, and professional rite of passage precludes detailed and careful attention to "temporal and historical processes" (p. 9). Although the importance of history may seem to be a commonplace in contemporary anthropology, Thomas argues that anthropologists have yet to contemplate the radical implications of historical temporalization. They continue to work with a dichotomy between history and culture in which culture is seen as a

structured order that resists change and persists despite fundamental social transformations. Thus, "a definite opposition is sustained between an authentic and more or less unitary traditional entity, of great richness and cultural complexity, and on the other hand a heterogeneous, relatively uninteresting and unproblematic intrusion of persons such as missionaries and settlers, and innovations such as cash crops and items of European technology" (p. 11). Excluded from such oppositions is any consideration of the formation of apparently enduring cultural forms within the context of contact, exploration, settlement, trade, and colonization.

Having removed small-scale societies from historical time, anthropologists have reconfigured them within other (evolutionary or world-systemic) temporal orders. Thomas critically examines the assumptions, procedures, and claims of well-known examples of such reconfigurations in Oceania—Irving Goldman's synthetic organization of "ancient" Polynesian societies along evolutionary axes and Jonathan Friedman's and Kajsa Ekholm's regional and global systems approach to island societies. In each case, Thomas uses explorers' accounts, missionaries' reports, diaries and memoirs, and colonial reports to reinterpret anthropologists' conclusions regarding political processes and social relations in specific societies, especially Marquesas, about which Thomas has written elsewhere in depth. These reinterpretations lead Thomas to one of his most important and problematic assertions. Showing how early ethnographers established their own authority by systematically excluding and delegitimizing earlier descriptive accounts written by explorers, missionaries, and colonial officers, Thomas argues that anthropologists need to reconsider the privileged status of ethnography and to use earlier accounts in their historical reconstructions.

In the main, Thomas is persuasive here. Clearly, the ethnographic method and form facilitate the removal of actual societies from historical time and reproduce the dichotomies Thomas rejects. Furthermore, anthropologists need to consider a wider range of archival and oral historical sources, including material published by missionaries and explorers, when they write cultural histories. Nonetheless, as Thomas recognizes, each of these kinds of narrative has its own forms, rhetorical devices, and intended audiences. They can be used by historically minded anthropologists, but they must be interrogated with care. Thomas clearly recognizes this, but his book does not address this problem in detail, and we do not learn enough about his own procedures of analysis and interrogation of sources to evaluate his brief reinterpretations. Thomas convinces us of the necessity of writing cultural history with new methods and sources, and on this ground his essay succeeds. Having convinced us, however, his book ends too soon. He does not take us into a critical examination of the

sources and methods he proposes, based on his own experience of historical reconstruction in Marquesas.

Nonetheless, this is an important and stimulating commentary on the present intersection of anthropology and history. The problems that it raises are some of the most important, and most frequently ignored, issues to be encountered in such an intersection.

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JEAN-CLAUDE SCHMITT. *La Raison des gestes dans l'Occident médiéval*. (Bibliothèque des Histoires.) Paris: Gallimard. 1990. Pp. 432. 185 fr.

Jean-Claude Schmitt, one of the leading exponents of the *Annales* school, has produced a large and important book that shows clearly the strengths and weaknesses of that much discussed approach to history. His subject is not merely "gesture" (*gestus*) but also ritual, symbol, comportment, deeds (*gesta* or *res gestae*), and the body. The story he tells is not primarily of the actual rituals and behaviors that were so important in establishing relationships, hierarchies, and communities in the Middle Ages; it is rather of the theory of gestures, which is to him synonymous with rationalizing or controlling (and sometimes even with repressing) them. As background, Schmitt recounts the eclipse of antique theories, which treated gesture as part of rhetoric, by an early medieval "mentality" in which the deeds or rituals of community were dominant. Most of the book focuses on the emergence in the twelfth century of a new interest in the details of religious comportment such as is manifested in Hugh of St. Victor's popular treatise for novices, and on the beginnings of the escape of theories of conduct from a religious context. Although wonderfully rich and imaginative in its use of evidence, the treatment of the twelfth and thirteenth centuries tends to break apart into a series of essays on such topics as Dominican prayer positions, representations of stigmata, manual labor, disputes over the eucharist, or changing attitudes toward the *jongleur*. One is puzzled as to why some of them are treated at such length, whereas other topics that appear more relevant—especially the rituals of personal subordination and property transfer so crucial in the feudal world—are barely mentioned.

Schmitt is too sophisticated to ignore the problems of writing the history of something that is, by definition, ephemeral—something frozen in texts only by violating its essence, which is motion. He is also too sophisticated to claim that there is a univocal "gesture" behind his case studies. Among the book's most attractive subtleties is the author's awareness that there cannot be a history of attitudes toward gestures because neither that to which the word *gestus* refers nor what we historians denominate "gesture" is the same thing in the sixth century and the thirteenth.

Nonetheless, something (is it the influence of Michel Foucault?) seems here to have derailed the *Annaliste* attempt to illuminate ordinary life by reading familiar texts in new ways. Much appears that is, by any definition, merely an account of persons behaving, whereas evidence (such as charters, chronicles, laws, religious treatises) of the observed impact of specific gestures seems curiously absent. Religious texts are quoted that categorize and analyze external comportment, but they are not placed in the context of the new twelfth-century understanding of a dialectic between the inner and outer *homo*. Perhaps focusing on the words that discipline behavior rather than on the behavior itself was the only way Schmitt could control a diffuse and difficult subject. But both the theory of gesture and the expressiveness of body and ritual in the high Middle Ages seem to lack their full complexity in his account.

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RODERICK PHILLIPS. *Putting Asunder: A History of Divorce in Western Society*. New York: Cambridge University Press. 1988. Pp. xx, 672. \$39.50.

Roderick Phillips's book is a massive investigation of changes in the legal status and frequency of divorce in Western society. The study ranges geographically from Australasia to Scandinavia and chronologically from the Middle Ages to the late twentieth century, but it focuses on developments in the United States, Britain, and France and on the sixteenth and nineteenth centuries, when the possibility of divorce and the grounds on which it could be sought were subject to intense debate and change.

First and foremost, this is a study of the arguments advanced for and against divorce over the past five centuries and the gradual liberalization of divorce legislation in France, England, and the United States. Phillips lays out sixteenth-century Roman Catholic doctrine on divorce, the Protestant challenge to that doctrine, the extremely restrictive divorce laws in seventeenth- and eighteenth-century England and the American colonies, the remarkably broad and liberal provisions of the divorce law passed during the French revolution (a law that was abrogated entirely in 1816, making divorce illegal again in France until 1884), and the gradual broadening of the grounds for divorce in the American states, Britain, and France throughout the nineteenth and twentieth centuries.

Because divorce was a state rather than a federal matter in the United States and no two American colonies or states agreed on the grounds for divorce until the late twentieth century, this study is more complex than the focus on three nation-states might indicate. Wisely choosing not to lay out all of the state laws on divorce, Phillips looks for trends and regional differences in the United States. Nevertheless, the

book contains a host of interesting examples. We learn, for instance, that in 1843 in Pennsylvania a wife's insanity, but not that of a husband, became grounds for divorce, and in Connecticut habitual drunkenness was added to the acceptable grounds for divorce. Meanwhile, in North Carolina, the guilty party in a divorce could not remarry during the lifetime of the innocent party, and a man or woman who remarried "in contravention of this stipulation was guilty of bigamy, a crime that carried the death penalty" (p. 448).

Conceptually, Phillips distinguishes divorce from marital breakdown, a sensible distinction that allows him not only to discuss the reasons why married couples might have stayed together in earlier centuries, even if divorce had been available, but also to raise questions about the causes and significance of the twentieth-century increase in divorce. Focusing on the eighteenth-century family economy, Phillips analyzes the economic necessity of marriage for most people, and the disincentive that family occupations and low wages provided for seeking separation or divorce. Not all marriages did survive, however, even when divorce was virtually nonexistent, and Phillips details a variety of ways in which marriages could end, ranging from formal separation (which meant separate dwellings but no remarriage) to desertion, wife sale, bigamy, murder, and suicide.

The most archivally based sections of the book are those on France. Building on his earlier work, Phillips details the revolutionary divorce law of France, the extent to which it was used, and the socioeconomic status of the people who sought divorces between 1792 and 1816. Although Phillips makes several attempts to be sensitive to gender in his analysis of divorce, one is struck by his lack of attention to the fact that 71 percent of the divorces granted under the 1792 law were sought by women, especially in light of his argument that women's low earning capacity made it more difficult for them than for men to survive outside of a marriage partnership. Also missing is any systematic analysis of the assumptions about gender and domesticity that are embodied in nineteenth-century divorce laws and any explication of the arguments of French and British feminists for the legalization or liberalization of divorce in their countries. It is true, as Phillips notes, that divorce was not a major feminist issue, but a study this large surely should include some of the arguments women posed for and against divorce.

It seems odd in a book of this size to wish for more, but one does. Scholars interested in one rather than in all three of the countries highlighted will want more detailed information. Historians interested in the history of women will find considerable information about the content of laws and their gender inequities but will want more information about the sources of these disparities, as well as the varied impact of divorce on women and men, and the

reasons why women did or did not support divorce legislation and did or did not seek divorces.

Although it leaves a variety of questions unasked and unanswered, this book should become the standard introduction to the question of divorce in Western society. It lays out in clear, well-crafted prose the legal developments; the intellectual, theological, and political issues involved in amending and liberalizing divorce laws in the Western world; and the social context within which these debates, and divorce itself, took place in Western society. That questions remain to be answered simply points up the dauntingly large nature of Phillips's subject and the impossibility of exhausting it in one book.

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JEAN-CHARLES SOURNIA. *A History of Alcoholism*. Foreword by ROY PORTER. Translated by NICK HINDLEY and GARETH STANTON. Cambridge, Mass.: Basil Blackwell. 1990. Pp. xix, 232. \$29.95.

This book tells the story of the excessive use of alcohol in all times and in all places. The task is too ambitious, and a sprawling approach leaves much material without integration into an overall framework. Despite these limitations, Jean-Charles Sournia's book is valuable. The research in French sources is excellent, and this translation by Nick Hindley and Gareth Stanton makes the research accessible to Americans. The book is a fine guide to evolving French medical theory and practice concerning the diagnosis and treatment of persons who have abused alcoholic beverages during the past two hundred years.

Between 1750 and 1850 the French, as well as most other peoples in the world, cared little about the overuse of alcoholic beverages. In the late 1700s, however, physicians such as the American Benjamin Rush and the Englishman Thomas Trotter observed that many patients, including those admitted to the newly built mental hospitals, suffered from overuse of distilled spirits.

The French suffered less, because they primarily drank, then as now, wine. Wine contained little alcohol and produced fewer ills than did hard liquor. Although Rush and Trotter called the overuse of alcohol an illness, only in the mid-1800s did European physicians, influenced by the Swede Magnus Huss, generally adopt a disease concept of alcoholism. This concept described overuse as a medical problem with physical, mental, social, and spiritual components. The patient was the victim of a disease, not an immoral social deviant. In France the word alcoholism was coined in 1849. From 1850 to 1950 the disease concept, in many variations, held sway in France and throughout the Western world. The new view led to intense medical and scientific investiga-

tions, to moral campaigns against a disease-causing substance, and to government control of production, sale, and use. Despite these trends, French consumption of all types of alcohol rose. During the 1870s, when phylloxera nearly ruined the wine industry, the drinking of spirits soared.

In the late 1800s, physicians and others collected data that associated heavy drinking with crime, poverty, mental illness, and social disorder. Abuse of alcohol was said to produce children and grandchildren susceptible to overuse. This social pathology led to the creation of France's first anti-alcohol organization in 1872. Given the importance of wine to the French, the anti-alcohol movement had little impact on drinking habits. Lobbying, however, did increase government regulation and taxes.

Until 1900, most French physicians blamed such medical problems of excessive use as cirrhosis of the liver on hard liquor. Wine, to the French, was more a food than an alcoholic beverage. Alcoholism was largely considered to be a lower-class problem. Its presence in the middle class was invisible. During World War II, little alcohol was available, and incidents of cirrhosis and admissions to mental hospitals declined markedly. Connections between consumption and health were finally established across class lines.

After 1950, French physicians approached patients on a more holistic basis. Most doctors rejected a moral vision of personal responsibility. Instead, they accepted a disease theory of alcoholism. Scientific research provided interesting new insights. For example, some investigators suspected that a genetic component explained susceptibility to excessive use. Some Southeast Asians had little of one enzyme that made drinking possible. On the other hand, other research threatened the disease concept. Individual variation in tolerances prevented the establishment of boundaries for a pathology called alcoholism. Can a disease lack a bounded pathology?

In recent years alcohol consumption patterns in the industrialized countries have been converging. Indeed, people all over the world often prefer imported beverages to traditional local ones. Alcohol consumption, therefore, is continuing, and the social and medical problems associated with excessive use are likely to remain significant.

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GEORGE L. MOSSE. *Fallen Soldiers: Reshaping the Memory of the World Wars*. New York: Oxford University Press. 1990. Pp. vi, 264. \$22.95.

George L. Mosse, who has inspired a generation of younger scholars with his pioneering work on nationalism, National Socialist culture, and anti-Semitism, has long been fascinated with the interplay between myth and reality in modern history. In this work he

employs the "myth of the war experience" as the leitmotif of historical continuity between the wars, a cultural foundation in which the memory of fallen heroes inspires national resurgence and future deeds of greatness. Mosse employs this theme as a vehicle to understand the troubling acceptance of mass death and terror as natural components of modern warfare. He also poses the question whether the mythology has contributed to the escalating cycle of terror and mass violence in our own day. Although the book is based on the German experience, the author casts his net widely, drawing parallels among the main combatants in World War I.

Mosse demonstrates that the new concept of the nation-in-arms began with the French revolution and became firmly established in the German wars of liberation against Napoleon. Almost from the beginning, the aesthetics of nationalism in the form of poetry, song, and art lent a sacred aura to traditional representations of armed military power. A century later, during the Great War, this had developed into a civic religion, blessed by the ecstatic front poetry of Ernst Jünger and Wilfred Owen and given universal expression by Josef Magnus Wehner, who declared in *Sieben vor Verdun* that "we went over the top into timelessness." Christian institutions and symbols reinforced the development of the secular religion of nationalism, lending the beauty of Christian liturgy, the reassurance of resurrection for the fallen heroes, and the comfort of eternal rest in military cemeteries, which became national shrines.

The work rests on the basic tenet that World War I was a fundamentally different kind of war, which combined both ugliness and beauty. Freed from the dull mediocrity of bourgeois materialism, the youth of Europe went into the Great War in a state of exaltation, as if partaking of the Blessed Sacrament. They were made whole in the grand camaraderie of the front. The cult of the fallen soldier—which reached its apogee in Walter Flex's *The Wanderer between Two Worlds* (1917)—was conceived when this ill-fated dream gave way to enormous slaughter. The Easter of resurrection promised the heroes eternal rest in the arms of Christ, as portrayed in *The Apotheosis of the Fallen*, painted in the Italian war cemetery at Redipuglia.

After the war, the legitimizing myth of national sacrifice grew in importance, transforming the horror of the war into resplendent national mythology. For the victors, this development made the agony of death more acceptable. But for the vanquished, who faced not only personal trauma but also national humiliation as a result of Versailles, the "myth of the front experience" became an essential component of German self-definition.

The author analyzes the influence of the war in the political arena as well as in high and popular culture. Chapters dealing with the appropriation of nature and the trivialization of the war are enormously suggestive. Enthusiasm for flying and mountaineer-

ing reached cult proportions, and beer hall kitsch competed for the public's attention with significant literary achievement. The Great War brutalized politics, as the paramilitary units of the radical parties became political shock troops. Even the language of the front suffused intemperate public debate. By World War II, both fascists and Stalinists had raised the threshold of brutality to a new level of terror. After 1945, no mythology could cloak the horror of mass liquidation perpetrated on Nazi racial and Marxist class foundations.

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DAVID HAMER. *New Towns in the New World: Images and Perceptions of the Nineteenth-Century Urban Frontier*. (The Columbia History of Urban Life.) New York: Columbia University Press. 1990. Pp. ix, 328. \$35.00.

This book is a major statement of comparative history. Rather than looking at the growth of towns on any one frontier, as have Richard Wade and John Reys in the United States and Norman Harper in Australia, David Hamer sets out to compare perceptions in his native New Zealand with those in the United States, Australia, and Canada and in the process has consulted the major archival libraries in all four countries.

Towns, along with the recognition of an "urban frontier," came with the first colonial settlements in these four countries. Among the regions, however, there were important differences, especially in the government's role in the founding of towns. In Australia, partly because the government held back land to increase the price for revenue, the new populations concentrated in a few large cities.

Everywhere the images of early towns created by travelers pictured dull, repetitive, gridlocked places in which it was nearly impossible to walk for pleasure. In contrast, boosterism, almost a moral imperative, was rampant, manipulating visitors and would-be visitors alike. Its optimism and utopianism were often coverups for a lack of community. Capitalism was generally credited as the cause of rapid development, with labor's contribution and poverty's presence seldom mentioned.

The usual intent was to transplant frontier towns in existing patterns. Yet differences were early perceived—greater available space, a lack of town centers, and rapidity of growth. Towns in the United States developed much more briskly than those in Canada, a result, it was thought, of greater free enterprise and the availability of credit south of the border.

Australia in this book gets almost as much attention as does the United States. Expansion "down under" met often with frustration and disaster. A few cities such as Sydney, Melbourne, and Adelaide remained

constant and dominant, unlike those in Canada and New Zealand, which had far less significant roles. Explanations for heavy Australian urbanization included the policies of the founders; land concentrations; immigrant preferences for urban life; the hinterland's pastoral economy, which required a small supply of labor; the weakness of small towns (unlike the American Middle West); and, most important, the forbidding nature of the vast, flat expanses of the outback.

It was the future for which frontier towns lived. Sometimes the future was a "relief from the miseries and confusion of the present" (p. 175) and was related to spatial assumptions, where grids were imposed on the environment and bird's-eye views showed wide streets as indications of what was to come. Most townspeople hoped for a rapid triumph of the town over the wilderness and saw that conquest as a moral one, which included the replacement of natives as symbols of savagery, useful only in underscoring the progress made by the town.

Such are some of the images discussed by Hamer. In the process he is occasionally repetitious, especially in the matter of boosterism and the import of towns to early colonization. The book lacks a sense of theory in which the themes might be drawn together. Hamer needs to take a stand rather than assume an "on the other hand" approach. Some of the theories seem doubtful; for example, the contention that frontier towns had no interest in their history except as a tool to prove the rapidity of the move to the modern. How often were fires welcomed because they destroyed the past?

These are not major questions. They only underscore the fact that the book is well worth reading. It is a thoughtful and imaginative contribution to comparative history.

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EFRAIM SHMUELI. *Seven Jewish Cultures: A Reinterpretation of Jewish History and Thought*. Translated by GILA SHMUELI. New York: Cambridge University Press. 1990. Pp. xv, 293. \$42.50.

Efraim Shmueli, who died in 1988, was a prolific writer whose work touched on many aspects of Jewish history and thought. He was also a maverick who never attained a regular professorship in a major institution of learning either in the United States or in the state of Israel. This volume, the translation of a somewhat longer Hebrew book, is Shmueli's most ambitious work. It is both challenging and, in more than one respect, problematic.

The thesis of the book is that Jewish history reveals no underlying essence, no scarlet thread that runs from biblical times to the present. Shmueli's argument is with those who have taken such a position for

scholarly or ideological reasons, thereby covering up change and innovation. The extent of change becomes readily apparent, according to Shmueli, when one compares seven discrete cultures that can be individuated from the continuum of Jewish history. These are the biblical, the talmudic, the poetic-philosophical, the mystical, the rabbinic, the culture of the Emancipation, and the national or Israeli culture. The first five he calls "cultures of faith"; the last two he deems secular.

Having introduced this scheme, Shmueli does not proceed to delineate the contours of each culture in turn. This is a disappointment, for the reader is never able to gain a comprehensive view of how one culture differs from another or how internal and external factors cause each instance of cultural succession. The fifth of the cultures, the rabbinic, receives almost no attention at all; the poetic element in the poetic-philosophical culture is quickly forgotten. As much metahistorian as historian, Shmueli presents instead a general theory of the rise and decline of cultures that recalls the nineteenth-century Galician philosopher of Jewish history Nachman Krochmal and of the nature of culture that draws heavily on the modern theorist Pitirim Sorokin. He then focuses on how various Jewish cultures that succeeded biblical culture interpreted the Hebrew Scriptures, devoting the most attention to the Song of Songs and the pentateuchal commandments, where the hermeneutical differences are the most striking.

After considering a number of tensions characteristic of all of the cultures (such as universalism versus particularism), Shmueli analyzes the nature of historical knowledge in the various cultures. Here he adds little to Yosef Hayim Yerushalmi's widely circulated *Zakhor: Jewish History and Jewish Memory* (1982), except that he gives more attention to the "Jerusalem School" of Jewish historiography, founded by Ben Zion Dinur and Yitzhak Baer. Shmueli subjects this school, representative of the last of his seven cultures, to severe criticism for ignoring fundamental changes in order to obtain a false sense of continuity and for shaping periodization to fit ideological conviction. This brings Shmueli to his own approach, which he calls the theory of "historical perspectivism." It is a method that is supposed to be unique in avoiding essentialism and in giving full weight to historical change. Yet, in the end, Shmueli produces an essentialism of his own composed of a similarity of "motives and impulses" that binds the two modern cultures to the five traditional ones and thus avoids rupture.

This volume suffers from repeated, forced attempts at categorization. One could have as easily come up with fewer or more cultures and with different defining characteristics. Despite its claim to a unified vision, the book is in fact a series of loosely connected essays with a propensity to the tangential. Occasional insights and admirable venturesomeness cannot compensate for a necessarily futile attempt to

compress Jewish history into predetermined, rigid structures that fail to do justice to its complexity.

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DEREK WALLER. *The Pundits: British Exploration of Tibet and Central Asia*. Lexington: University Press of Kentucky. 1990. Pp. viii, 327. \$30.00.

In the eighteenth and nineteenth centuries, British expansion in Asia was propelled by a "forward policy": a belief that the British could, and should, go wherever they pleased. Because the indigenous people did not share in this notion, the British were frustrated in their efforts to penetrate areas north of the Indian plain, a frustration heightened when the Russians began to show interest in the same region.

Never seriously entertaining the preferences of the local people to be left alone, Britain was determined to explore these regions in order to expand its territorial reach, set boundaries, create buffer zones, and search for new trade opportunities even as Europeans continued to be unable to penetrate these areas and return unscathed. This book is about the British solution to the dilemma through the use of native inhabitants as proxies.

The ideal surrogate was a local inhabitant, preferably a trader or pilgrim familiar with the region, whose ethnicity, religion, and language was similar to the people among whom he was to travel. Finding individuals who fit this description, had the capacity to acquire the necessary training, and were willing to enlist in the British imperialist cause was no easy feat.

There was also considerable reluctance to employ such men because of official wariness of teaching them the necessary skills and allowing them access to the vital information they would gather. Racism was also a factor; some officials were unable to believe indigenous people capable of performing the specified tasks. (After one particularly successful mission in 1868, the Royal Geographical Society praised the combination of "native enterprise directed by English intelligence" [p. 52]). Only reluctantly did this opposition end when it was deemed there was no other way.

For the first time the remarkably successful exploits of these men have been properly told. These Muslim native secretaries (*munshis*) and educated Hindus (*pundits*) were trained to take measured paces, calculate longitude, latitude, and altitude, measure the grades of roads, and gather political, social, economic, and military information. They were equipped with geographical instruments hidden within ordinary objects such as walking sticks, rice bowls, and prayer wheels. Buddhist rosaries of 108 beads were redesigned into 100-bead rosaries with every tenth bead of a different shape to use in counting paces.

Left completely to their own devices on leaving British-held territory, these men were forced to overcome the most adverse circumstances, walking thousands of miles under hostile weather conditions in the rarified air of the "roof of the world" and constantly under threat of capture. These intrepid and brave souls explored eastern and western Turkestan, Nepal, northern Burma, Assam, and, especially, Tibet, providing the British government with critical information that allowed the complete domination of the region and aided in the British invasion of Tibet in 1903.

Derek Waller, through prodigious research on several continents, has produced a fascinating and historically important book about the frontier policies of the British empire, policies bearing some resonance even today as controversy over frontiers (the Durand Line, the McMahon Line) in the region continues. The superb maps allow the reader to follow clearly the path of these pundits as they marched across Central Asia counting their paces and taking their measurements. Most important, in his complete account of their selection, the questioning of their competence, and the graphic descriptions of their exploits, Waller has given these men their proper due.

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Empire State College

ROBERT ALDRICH. *The French Presence in the South Pacific, 1842–1940*. Honolulu: University of Hawaii Press. 1990. Pp. xii, 387. \$35.00.

This book can be admired as a model of historical synthesis. It is thorough, careful in its judgments, analytical in approach, and based both on wide reading of the secondary literature and on original archival research. Above all, it is balanced and detached in the best sense of historical scholarship, although the author does not hesitate to express a judgment or reflect on events and previous interpretations. As a survey of the French presence in the South Pacific during the century from 1842 to 1940, Robert Aldrich's history complements and goes beyond the previous major study in English, Virginia Thompson and Richard Adloff's *French Pacific Islands: French Polynesia and New Caledonia* (1971).

Aldrich provides neither a traditional, political survey of the French in the South Pacific nor a strictly chronological approach. Instead, emphasis is on certain thematic elements that varied in importance during the course of the century. These dimensions include religious and cultural contacts between French and indigenous populations, military and strategic considerations, economic and commercial temptations, and the political and administrative aspects of French colonial rule.

The story is one of expectation and disappoint-

ment. From the first establishment of a French presence on the Pacific islands, quarrels among Protestant and Catholic missionaries and between missionaries and naval officers, who represented the reality of French power, characterized the French experience. The commercial and economic value of France's Pacific role proved beneficial to a few larger interests but always lagged behind the enterprises of British, Germans, and Americans, and the lot of the ordinary settler was harsh and distinctly unromantic. A theme of metropolitan neglect runs as a constant complaint through this history and marked the outlook of the settler population. Despite the promotional enthusiasm of the Oceanic lobby in France, the French empire in the Pacific seldom fulfilled supporters' hopes for development. Aldrich is evenly critical of the settlers, of the missionaries, and of administrators who felt themselves condemned to Pacific exile. On the other hand, French naval officers held a more realistic view of what might be gained from the Pacific colonies.

A specialist in French history, Aldrich has made good use of approaches by cultural anthropology to interactions between settler and indigenous populations. The account of the way in which the Melanesians of New Caledonia lost their land and space is done with sensitivity and insight. As a specialist in economic history, Aldrich gives a fine account of the economic limitations of the French presence in the Pacific. And, as a shrewd observer, he notes the finite limits of the French Pacific empire, seen after the turn of the century in more or less open acknowledgment, not sustained by any enlightened French colonial policy, of the French islands' dependence economically and politically on relations with the other Pacific powers, notably Australia, New Zealand, the United States, and Japan. French rivalry with "the Anglo-Saxons" is another leitmotif. With the onset of depression and the outbreak of World War II, a new and serious crisis faced France in the Pacific.

The only disappointment with this fine study may be found in a few errors (Paul Leroy-Beaulieu becomes Pierre) that might have been avoided with careful proofreading, and the index is sketchy. The bibliography, however, offers an excellent introduction to recent contributions by French and Australian scholars.

KIM MUNHOLLAND
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ELSPETH WHITNEY. *Paradise Restored: The Mechanical Arts from Antiquity through the Thirteenth Century*. (Transactions of the American Philosophical Society, number 1.) Philadelphia: American Philosophical Society. 1990. Pp. 169. \$20.00.

For the past three quarters of a century, much highly original research has been devoted to medieval tech-

nology, the value placed on it, its social impact, its ethical implications, its causes, and its relation to the modern world. Elspeth Whitney begins her study with a critical review of this literature and points up several important problems that arise from it—the most important of which is the medieval (and to some extent ancient) cultural attitudes toward the crafts, or mechanical arts—in order to give us a “detailed and coherent assessment of what might properly be called the philosophy of technology in the Middle Ages” (p. 1). After a chapter reminding us of the ambivalence of classical attitudes toward the crafts and that “technological arts were often described as a basic part of knowledge in antiquity” (p. 50), she begins her exposition and analysis of classifications of the sciences from Isidore of Seville to Roger Bacon, which constitutes the major portion of the book. She brings a lively and original intelligence to this task and gleans implications of fairly well-known texts that shed considerable light on her problem. The heart of the book is chapter 4, on Hugh of St. Victor and his followers. Although Hugh’s classification of the sciences in his *Didascalicon* has frequently been studied, Whitney makes some valuable original observations on it: that Hugh eschewed the distinction between illiberal and liberal crafts common in antiquity and among the church fathers and used the term *artes mechanicae* for all crafts; that his ideas “developed out of a specific philosophical milieu” (p. 88); that he was not an isolated thinker on these matters; and that “through its relationship to man’s final end, the pursuit of the mechanical arts [in Hugh’s thought] acquired religious and moral sanction” (p. 90). She follows these points with a very valuable section tracing the influence of Hugh’s classification up to the end of the thirteenth century. Chapter 5 investigates an alternative tradition, deriving from Aristotelian and Arabic thought, “in which crafts were assigned a more purely secular function as practical parts of theoretical science” (p. 129). I wish she had devoted more thought and space to this tradition, because it is in some ways more important than the incorporation of the crafts into the Christian life, about which she writes so well in the preceding chapter. The brief conclusion, in addition to summarizing points previously made in the text, holds that, although the thirteenth-century rejection of the manual arts by the monastic community tended to secularize them, “writers on the mechanical arts continued to justify those arts on both religious and secular grounds, not as works of the hands but as intellectual work. . . . Nor did twelfth- and thirteenth-century thinkers have a clearly worked out view of science as founded on mechanical principles and primarily concerned to manipulate the material world for the practical benefit of mankind” (p. 149).

This book is praiseworthy for not going beyond the sources, for drawing conclusions based on the extant evidence, and for not introducing the troublesome notion of causality into the discussion. It is a valuable

summary and criticism of a vast amount of historical work and will be read with profit by anyone interested in the Western European attitude toward the value of the manual arts.

RICHARD C. DALES
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JOEL MOKYR. *The Lever of Riches: Technological Creativity and Economic Progress*. New York: Oxford University Press. 1990. Pp. ix, 349. \$24.95.

This is an important book that may well win a Pulitzer or other significant prize. The thesis is succinctly summarized in the subtitle. Joel Mokyr, an unabashed advocate of material progress, is an economic historian whose previous works have used cliometric techniques and emphasized economic theory. In this book, happily for the general reader, he departs from that methodology to produce clear and vigorous prose with generous doses of common sense. For example, he candidly admits to ambiguities in the historical record and to the limits of his own and general knowledge. The text is refreshingly free of jargon, and technical concepts are explained in plain English.

The book is organized in four parts of unequal length. Part 1 consists of a single introductory chapter in which Mokyr states his theme. Part 2, entitled “Narrative,” contains five chapters sketching the history of technology from classical antiquity to the late nineteenth century. (Mokyr wisely ends his narrative with the outbreak of World War I.) Part 3, with four chapters, is devoted to “analysis and comparisons,” in particular a comparison of classical and medieval technology, in which the latter gets its due; a comparison of China and Europe; and a comparison of Britain and continental Europe in the eighteenth and nineteenth centuries. Part 4 contains a long essay analogizing technological change and organic evolution and a brief epilogue recapitulating the argument. A lengthy bibliography and a detailed index complete the book. The narrative portion of the work is amply illustrated, although some of the illustrations call for more explanation than is provided.

In spite of my strong admiration for the book, I must confess to some minor disappointments and one major disagreement. By beginning his narrative with classical antiquity, Mokyr fails to emphasize that almost all of the major elements of technology that served ancient civilizations—domesticated plants and animals, textiles, pottery, metallurgy, the wheel, the lever (which he mistakenly attributes to Archimedes), sailing ships, and such—had been invented or discovered before the dawn of recorded history. The narrative is very rich in parts—for example, his discussion of medieval technology, in which he relies heavily on Lynn White and other recent revisionists—but rather thin in others, especially the late nineteenth century, as if Mokyr were literally running out

of steam. There are also minor errors of fact and chronology, but these are not worth detailing given the scope and importance of his main theme.

The major flaw in the work—and it is a large one—is the central role Mokyr assigns to the traditional, misnamed, and misconceived industrial revolution. He even tries to deify it with initial capital letters. Mokyr is certainly familiar with the recent revisionist literature that convincingly demonstrates that there was no marked acceleration of economic growth in mid- or late-eighteenth-century Britain and that the organizational and institutional changes traditionally associated with that non-event were drawn out over a period of at least two centuries. In his penultimate chapter Mokyr tries to make a case for the uniqueness of the period from 1750 to 1850 in the history of technology by analogy with recent developments in evolutionary biology, but, as he admits, analogies can only be suggestive, not conclusive. It is difficult to understand why he chooses to turn his back on recent new knowledge, like some fundamentalist ayatollah of economic history. I will not suggest that it is because of a vested interest in the term (he has edited a textbook, *The Economics of the Industrial Revolution*), but it is significant that in an otherwise very full bibliography there are some glaring omissions—and not only of works by me—that can only be deliberate. Mokyr rightly claims that one of the conditions for technological advance is a degree of tolerance and open-mindedness. It is a pity that his own work does not exemplify those qualities.

RONDO CAMERON
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ALON KADISH. *Historians, Economists, and Economic History*. New York: Routledge. 1989. Pp. xii, 297. \$65.00.

Alon Kadish provides, in his own words, a blow-by-blow account of the emergence of economic history as an academic discipline in England in the late nineteenth and early twentieth centuries. The focus is on key individuals and institutional developments. His objective is to show that the “notion of linear intellectual genealogy is unsatisfactory” (p. ix). In other words, an interaction of individual scholars—Thorold Rogers, William Stubbs, and Arnold Toynbee at Oxford; William Cunningham, J. H. Clapham, J. N. Keynes, Alfred Marshall, and William Ashley at Cambridge—in the context of the intellectual currents of the times led to the split with mainstream economics as shaped by Marshall and to the consequent evolution of economic history as a separate subdiscipline. The controversies focused on the role of theory in history and the role of history in theory.

In many respects, the century-old controversies that Kadish describes have a familiar ring. Is economic theory essential to writing good economic history? Is economic history essential to formulating good economic theory? The answer to both questions

is equivocal. Economic theory is a necessary but not sufficient condition for writing good economic history. And economic history, properly conceived, is essential to economic theory.

Understanding how an economy works (price or microeconomic theory) is a necessary condition to writing economic history, but economic theory is static in its implications, and the key to good economic history is explaining change over time—something missing in economic theory. It is precisely that missing ingredient that should be the contribution that economic history could make to improving economic theory. More than anything else, Kadish's story is one of missed opportunities that continue to the present day. Economic theory has become more and more mathematical, formal, and precise about less and less. Economic history in England has gone its own way with separate departments without even the leavening influence of rigorous price theory underpinnings. And, in the United States, where economic historians have slavishly imitated economists, they have failed to add the essential dimension of explaining change over time, which would make their work a contribution to a more relevant and dynamic economic theory.

Kadish's study ends with the founding of the Economic History Society in 1926. The publication of its organ *The Economic History Review* in 1927 functionally replaced the need for the economic history supplements of the *Economic Journal* (the official journal of the Royal Economic Society). This book is essential reading for those who are interested in understanding the history of economic history.

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ISAAC COHEN. *American Management and British Labor: A Comparative Study of the Cotton Spinning Industry*. (Contributions in Economics and Economic History, number 109.) New York: Greenwood. 1990. Pp. vi, 250. \$45.00.

This study of nineteenth-century British and American cotton mills and their workers focuses in large measure on mule spinning, a skill-intensive craft devalued gradually by technical change and eclipsed more rapidly on this side of the Atlantic. In seeking to illuminate national contrasts between millowners' and workers' actions, Isaac Cohen exposes the conditions that favored “workers' control” in Britain and “management control” in the United States (p. 1), despite parallel mechanical advances. Conceptually, the discussion relies on a distinction between craft and mass production, relationships between technologies and organizational practices, and the dynamics of “craft control” in manufacturing (p. 5). The mule's product versatility made its adoption ideal for free-standing spinning mills in Britain, permitting the application

of skill to diverse yarns made from varied mixes of raw cotton for multiple clients. This market setting conditioned factory relations that made room for the craftsman's autonomy, minimal supervision, and accommodation with unions. Even as skill was diluted, competition among firms precluded unified anti-union stances and preserved craft control through the century's end.

The American environment was sharply different. Here the market had long called for narrow ranges of coarse staples. Leading firms integrated spinning and weaving, with mules being used for warp making alongside frames that spun softer filling yarn, the latter work done by women and youths. This focus on mass production restricted use of mules' flexibility and, more important, absent craft traditions, a format that did not tolerate workers' efforts to establish craft control inside the mill. Cohen employs the well-known case of British immigrant mule spinners' Fall River strikes (to "regain" shop authority that they had exercised abroad) as dramatic evidence of confrontation between the two approaches (p. 117). Crushing these walkouts in the 1870s through close interfirm collaboration, U.S. managers steadily replaced mules with improved frames, designed with mass output in mind, thereby eliminating fractious men in favor of "women and boys" who "cause[d] less trouble" (p. 131). The study closes with short comparisons of technology, organization, and craft control in the two nations' coal and iron and steel sectors.

Cohen has skillfully mined an extensive secondary literature, adding relevant research at critical points, to create a provocative international analysis. His concern with a wide range of economic, cultural, institutional, and technical issues is especially welcome, as are his clear explanations of machine operations. In the Fall River case and closing intersectoral contrasts, however, the market structure and issues of product and technical flexibility, key to the theme of craft versus mass production, all but disappear. Perhaps contrasting machinery building would have offered resonance with all three of Cohen's core concepts—machine tools versus sewing machines, for example, with Singer's three Scottish works illustrating the extension of mass strategies into craft heartlands. Given that the endnotes regularly add to the text, it is regrettable that they are separated from it. Cohen's approach to comparative studies of industry, technology, and labor, even if not pursued with as much consistency as I might have hoped, merits both our appreciation and our emulation, just as his complex argument deserves fuller exposition than feasible in such limited space.

PHILIP SCRANTON
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Camden

MICHAEL ADAS. *Machines as the Measure of Men: Science, Technology, and Ideologies of Western Dominance*. (Cornell Studies in Comparative History.) Ithaca: Cornell University Press. 1989. Pp. xii, 430. \$29.95.

"Remarkable" is an adjective that is most appropriate for this study. Broad in interpretation, rich in detail, and supported by a wealth of information—the footnotes give the appearance of the foundation of a nineteenth-century town house—Michael Adas's work will command the attention of every scholar of modern imperialism, every student of the broad subject of "technology."

The book is as clearly written as it is thoughtfully presented. Concerned with the ideology of imperialism, with the much-vaunted and ridiculed "civilizing mission," Adas demonstrates how significant technology, particularly the well-oiled and noisily articulated steam engine, was in the advance and advancement of European domination in what was—and still is—labeled as the "non-Western world."

Adas carefully constructs and develops where others have laid the initial groundwork. The pilot articles on technology and empire written by Henri Brunschwig and the more synthetic work of Donald Kendrick serve as prefatory statements to this study, which is concerned with the cultural impact of technology, not with its physical overlay.

Tracing the growing disparity between the machine-manufacturing West and the rest of the world from the eighteenth century on, Adas concentrates on the shift in attitude, on that new post-Enlightenment climate of opinion when broader cultural and philosophical distinctions gave way to those of things: weapons, conveyances, printing presses. He does not quote Hillaire Belloc's famous quip, "Whatever happens, we have got the Gattling gun and they have not," nor is his interpretation dipped in the cynicism that coats Belloc's couplet—and that can easily taint studies of imperialism. Adas offers an example of popular history at its very best, which is cultural history exquisitely constructed of detailed research, a well-designed overarching theme, and nicely polished prose.

If the book encounters any criticism, it may be that of overemphasis. The makers of empire and their apologists whom Adas assesses were chiefly those in high colonial positions overseas or polemicists and officials back at home, not the "bush" administrators. The powerful ambivalence between the industrial and the Edenic, which is found in colonial thought, expressed in the memoirs and commentary of those who were abroad, and which affected imperialist ideology, is not considered. Robert Delavignette, the great twentieth-century French colonial administrator, is but one of many for whom that ambivalence was important. He once described his residence in West Africa as that of a "gentleman farmer" and commented that there, where he was, were factory and truck-rutted roads, "yet there is something else, and it is the land" (Delavignette, *Afrique occidentale française* [1931], p. 5).

Although the reviewer is cautioned against the use of the superlative, a book occasionally commands such praise. This work is such a one. It will long be pivotal in all discussions that revolve around the technology and culture of modern European expansion. In sum, this is a most compelling, splendid book.

RAYMOND F. BETTS
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JOHN GIMBEL. *Science, Technology, and Reparations: Exploitation and Plunder in Postwar Germany*. Stanford: Stanford University Press. 1990. Pp. xv, 280. \$29.50.

John Gimbel has made a major contribution, through two earlier studies and a number of articles, to our understanding of what the United States intended to do, and did, in occupied Germany after World War II. He has done so with a critical eye, questioning some of the myths concerning the politics and economics of Allied occupation policy, which have found their way into memoirs and autobiographical publications of various American actors. Gimbel's new book now looks into the hitherto relatively uncontroversial area of German reparations. Here the received wisdom, until he came along, has been that only the Soviets extracted heavy reparations from their zone of occupation. The Western Allies, by contrast, learning the lessons of the post-1918 period, quickly abandoned the idea of payments in cash and kind. This book now raises the question of "a much more comprehensive and systematic 'intellectual reparations' program to exploit German scientific and technical know-how not only for military purposes, but also for the benefit of American science and industry" (p. vii). That these "hidden" or "creeping" reparations should be taken seriously is indicated by the (deliberately?) pointed subtitle of Gimbel's careful study. To him "the program was certainly remarkable for its scope, for its systematic application, and for its failure to distinguish between war booty, reparations, and loot" (p. 169).

To support his case, the author has evaluated the records of the U.S. technical intelligence (FIAT) units, which moved into the factories and offices of German enterprises at the end of the war in search of equipment, patents, scientific research, and technical know-how of potential usefulness to American industry. This is therefore a more broadly based study than Tom Bower's angry *Paperclip Conspiracy* (1987), which dealt with the American "Hunt for Nazi Scientists." It was probably inevitable that all of these fascinating reports now sitting in the National Archives would be evaluated at some stage, and to this extent it is to be welcomed that Gimbel came in to bring to bear his intimate knowledge of the archives as well as his personal experiences as a former member of the U.S. occupation administration. He is also very good at analyzing the tensions that arose within the U.S. bureaucracy over these policies.

Nevertheless, the question remains just how important these "intellectual reparations" were to American industry. The Germans, it is true, were ahead in some areas, such as radio condensers, applied chemistry, and twin-engine diesels, which were judged to be "far superior to anything that had been produced or planned in the States" (p. 23). But there were many other fields where the Germans were behind and soon keen to acquire American technologies and production techniques. Ultimately, Gimbel therefore pries open just a bit the door to German-American industrial relations both before and after 1945. Behind this door lies a much wider area for the historian of Western industry who is interested in what happened to those intellectual reparations from Germany. No doubt there were benefits, and few companies were presumably so short-sighted as the team of British car manufacturers who rejected the use of Volkswagen's technologies. They thought the "boxer-engined" Beetle too primitive and outdated, only to complain a few years later about Volkswagen's competition in lucrative third markets. No doubt many American companies also felt inhibited from using German patents without having paid for them. Thus a Hamburg industrialist was surprised to receive a visit in 1946 from two American colleagues who were interested in his machinery and actually offered a license fee rather than take the machinery away under Allied occupation law.

So Gimbel is absolutely right when, in his concluding passages, he refers to the FIAT program as a "conveyor-belt for future business connections" (p. 183). To reconstruct this network of contacts, many of which went back to the prewar period, will be a task of future research. In the meantime historians of postwar Germany and its reintegration into the West will read this book with much profit.

V. R. BERGHAHN
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BEATRICE HEUSER. *Western "Containment" Policies in the Cold War: The Yugoslav Case, 1948-53*. New York: Routledge. 1989. Pp. xx, 304. \$49.95.

Beatrice Heuser has carefully documented Western cold war "containment" policies and strategies as reflected in British, French, and American responses to the opportunity provided by the Tito-Stalin split of 1948 until Stalin's death in 1953. The study covers two time periods: June 1948 until June 1950, when Western hopes focused on a perceived Yugoslav defection as a form of national communism that might infect other East European Communist regimes, and June 1950 until March 1953, when such ideological considerations gave way to strategic benefits of Yugoslavia as a "defensive shield" for NATO's western and southern flanks.

The author acknowledges the methodological pitfalls involved in distinguishing between perceptions

and "reality" constructed on the basis of partial knowledge. That is a problem. In this regard, however, the end of the cold war is much more likely to be the beginning than the end of history. In the age of glasnost, one can hope that there will be an opportunity for Heuser to check her reconstruction of Stalin's reality against Soviet archives. In the meantime, this analysis is the best of its kind based on available Western archival sources.

This is a valuable book for many audiences. With regard to comparative foreign policy, the author makes a useful distinction between foreign policy decision makers and the institutions through which their decisions were translated into policies. In doing so, she provides insight into differing patterns of domestic-foreign policy linkages among the Western powers.

For students and scholars of American foreign policy, the book raises the provocative question of continuity in the mentality that emerges from this case study of Washington's policy toward Yugoslavia. The extent to which American stereotypes concerning Stalin's revolutionary ambitions blinded U.S. diplomats to his actual foreign policy agendas has more than a little relevance for our strategies in the Middle East. It is not out of the realm of possibility that the contradiction between American commitment to "liberation" and hatred of all things Communist in principle and Washington's willingness to do business with a Communist government, without "crude attempts to interfere in its internal affairs, if the Communist government was not a tool of Soviet expansionism" (p. 61), foreshadowed the statement of the luckless ambassador to Iraq who told Saddam Hussein that his differences with Kuwait were an inter-Arab problem.

Finally, those whose research centers on Yugoslavia will also benefit from this analysis of Yugoslav economic, military, and diplomatic interactions with Western governments. Heuser's account adds yet another dimension, especially to our understanding of the Yugoslav security strategy during the time of troubles that followed Yugoslavia's expulsion from the Cominform.

The author moves somewhat beyond her data to speculate that the Western alliance with Yugoslavia may have deterred Stalin from launching a preventive war against NATO; however, it would be hard not to agree with her conclusion that the main beneficiaries of these Western policies were the Yugoslavs themselves. In sum, this book is well researched, well written, and worth reading.

ROBIN ALISON REMINGTON
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PHILIP J. FUNIGIELLO. *American-Soviet Trade in the Cold War*. Chapel Hill: University of North Carolina Press. 1988. Pp. xii, 289. \$32.50.

The purpose of this book is to explain why and how the United States has applied trade sanctions toward the Soviet Union (primarily) as well as China and Eastern Europe since World War II and to evaluate the effectiveness of those sanctions. Introduced by a concise chapter that reviews U.S.-Soviet economic and diplomatic relations during the interwar period, the main body of the book is a well-documented account of the process of policy formulation, including the debates in the executive and legislative branches of the government under several administrations, from Dwight Eisenhower to Ronald Reagan. Philip J. Funigiello makes extensive use of documents declassified in the 1980s. The issues are placed in their contemporary context. The book is written clearly, concisely, on occasion even elegantly.

The principal issue in U.S. East-West trade has been, and remains today, how to walk the difficult line between export promotion and export control vis-à-vis a potential adversary. Each sale involves balancing the presumed strategic and economic value of a product to an adversary against the commercial worth to the United States of the sale and the prospect that the adversary could obtain the item from other sources. The net assessment of citizens, policy makers, legislators, and government departments is always determined by the views held in the ideological spectrum (as the author himself unwittingly demonstrates) and bureaucratic responsibility or vested interests. Therefore, it is not surprising that most business persons and government agencies charged with promoting trade have tended to make a case for the easing of restrictions; they are supported by those who believe that increased trade contributes to peace. On the other side are those who are more concerned with the military and economic might of the USSR and the purposes for which that power could be employed; these strategists have tended to opt for restricting exports.

One interesting insight is Eisenhower's balanced view on East-West trade, which he formulated at a time when it was fashionable to advocate a hard line and no differentiation among the Communist countries. In debating his secretary of defense, Charles Wilson, who argued that "one should not sell firearms to the Indians," Eisenhower held the view that "the last thing [we should do is to] force all the peripheral countries—the Baltic states, Poland, Czechoslovakia and the rest of them—to depend on Moscow for the rest of their lives. . . . If you trade with them, Charlie, you have got something pulling their interest your way . . . you just can't preach abstraction to a man who has to turn for his daily living in some other direction" (p. 76). Eisenhower was thus ahead of his time; he in fact advocated and quietly moved toward a policy that later became known as "building bridges" to Eastern Europe.

The book concludes with an epilogue, which is an

excellent summary of the author's main points. Funigiello's assessment notes that much of U.S. policy on export controls and the periodic economic sanctions imposed in response to certain Soviet actions have been prompted by domestic political considerations and "to give a visible demonstration to the rest of the world that the United States disapproves of Moscow's aggressive international behavior" (p. 218).

The one significant shortcoming of the book is the basis on which the author criticizes the restrictive aspects of U.S. economic policy. Instead of saying that he, for subjective reasons, would have preferred a different outcome on the "restrict or promote" debate, Funigiello sets up a straw man and then takes obvious enjoyment in knocking it down. The straw man is the criteria against which he judges the effectiveness of U.S. policy. Having noted that U.S. policy has been driven largely by domestic politics and by the desire to register, at least symbolically, disapproval of aggressive Soviet behavior, Funigiello then concludes that U.S. policy has been ineffective because "economic sanctions neither brought down the Soviet government . . . nor appreciably retarded its military capability" (p. 216). Ending the Soviet regime was not what postwar administrations expected from U.S. economic policy; on restriction of Soviet military power, experts disagree. The author's bias is also revealed by his choice of words and the occasionally loaded terminology, such as dubbing as "meaningless" economic sanctions imposed to show disapproval of Moscow or noting that "President Carter resorted to economic coercion [that is, embargoing certain U.S. exports] to achieve a political objective, with predictably disappointing results [my emphasis]" (p. 223).

I do not dispute the author's right to assess U.S. policy as he does. But opinion should be presented as opinion, not as if it were arrived at by applying criteria that are objective.

This book is a meticulous and very readable presentation and documentation of the formulation and implementation of U.S. economic policy toward the Soviet Union since World War I; Funigiello has written an important reference work of lasting value.

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JAMES SCHWOCH. *The American Radio Industry and Its Latin American Activities, 1900–1939*. (Illinois Studies in Communications.) Urbana: University of Illinois Press. 1990. Pp. xiii, 184. \$29.95.

This short study investigates the role of radio in the expansion of the United States into Latin America. It is not a history of radio or industry or diplomacy, nor does it offer any new interpretations beyond those of Susan Douglas, Hugh Aitken, and Walter LeFeber. The theme of the book is capitalistic expansion, in particular as exemplified by the U.S. radio industry in

Latin America. It begins with attempts in the early 1900s by U.S. Rubber and United Fruit to establish radio operations in Latin America in order to expand their commercial empires. The Amazon Wireless Company of U.S. Rubber failed in Brazil, but United Fruit was more successful in Central America, where there was a greater U.S. diplomatic and military presence and less European competition from Marconi and Telefunken. The U.S. government became more interested in dominating radio during World War I, especially with the navy taking over all broadcasting stations and absorbing fledgling companies such as Federal Telegraph. But there was conflict between private companies and government policy makers on the future of radio until the war ended.

The most interesting section of the book deals with the 1920s and the creation of a global communication system by a new cooperative government-industrial complex. One chapter deals with several important international communication conferences that saw U.S. dominance emerge. In preparation for the Washington Conference of 1920, the Commerce Department formed a radio committee to create policy, with Secretary of Commerce Joshua Alexander playing a key role for which Herbert Hoover is usually credited. Although the conference was a failure, American radio policy continued to develop at the Hotel Commodore meetings and was fully expressed publicly for the first time in the Babcock statement at the Mexico City conference of 1924. By 1937, the U.S. system of frequency allocation, which favored the technological expertise of the large U.S. research laboratories rather than national monopolies, became the world standard.

The last part of the book attempts to impose a theoretical construct on the story of radio's rise to global dominance. Using Fernand Braudel, James Schwach argues that the capitalist dynamic was more important than national policy. It would have been more rewarding for readers if the author had devoted more space to the historical narrative. The potentially interesting story of U.S. penetration of the radio market in Brazil and Argentina is truncated by a theoretical discussion of capitalist space and time. The history of the American-English-French-German consortium and its Radio Sud America effort is intriguing and merits a fuller treatment. Schwach has grounded his study on primary sources, especially the records in the National Archives of the State and Commerce departments, and the extensive footnotes and references to the secondary literature are useful.

STEVEN E. SCHOENHERR
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RUTH LEACOCK. *Requiem for Revolution: The United States and Brazil, 1961–1969*. (American Diplomatic History.) Kent, Ohio: Kent State University Press. 1990. Pp. xi, 317. Cloth \$30.00, paper \$18.50.

Spurred by the challenge of Castroism, liberal academics and members of the Eastern Establishment in the United States came together in the early 1960s to plot a strategy to block the spread of international communism in developing areas of the world. The problem, they decided, called for nothing less than a comprehensive campaign of nation building in countries at risk. One of the areas targeted, according to Ruth Leacock, was Brazil. Her effort to tell the story of that ill-starred attempt to bring forth a progressive, stable, anticommunist polity has produced a clearly written, informative history of U.S.-Brazilian relations during the administrations of John Kennedy and Lyndon Johnson.

In a richly detailed narrative of the nation's social and political history during the 1960s, Leacock describes how Brazil shifted from a philosophy of developmental nationalism under President Jânio Quadros to the doctrine of the national security state under President Humberto Castello Branco. Developmental nationalism called for the creation of a diversified, self-sufficient economy but depended on a definition of Brazil as a nation exploited by foreign, imperial powers, namely the United States. Quadros's approach involved a dramatic restriction of the activities and profits of multinationals such as ITT and an "independent" foreign policy. Under Quadros and his successor, João Goulart, Brazil attempted to establish closer relations with the developing countries of Asia and Africa, define international relations in economic rather than political terms, and stress the inter-American principles of nonintervention and self-determination.

Unfortunately for Quadros, Goulart, and Brazil, the rabidly anticommunist Kennedy administration perceived developmental nationalism as a threat to the economic and strategic interests of the United States, although, according to Leacock, the desire to block communist expansion was always more important in the minds of policy makers than protection of vested American interests. Initially, the Kennedy administration operated on the assumption that, despite the threat posed by developmental nationalism, it could not let Brazil go bankrupt. What resulted was a bizarre charade in which Quadros and Goulart played to the Brazilian Left in order to persuade it to acquiesce in the deflationary economic policies imposed on the nation by Washington in return for American aid. By the end of his term, Kennedy had decided that the business of the United States in Latin America was not social reform à la Walt Rostow and Arthur Schlesinger, Jr., but, rather, business. The Alliance for Progress, Kennedy's plan for a hemisphere-wide managed revolution, collapsed under the weight of Brazilian nationalism, the country's intractable fiscal problems, the vagaries of its politics, and the ability of American firms to arouse congressional and public opinion.

Johnson and Thomas Mann merely continued a policy that had begun under Kennedy. The military

coup that ousted Goulart and brought Castello Branco to power was an inevitable outcome of Washington's anticommunist obsession and the growing influence of the business community on the Latin American policy of the United States. Ambassador Lincoln Gordon, the Central Intelligence Agency, and the State Department deceived themselves into believing the coup was a democratic movement that had as its principal goal the prevention of a communist takeover. What emerged was a totalitarian regime that justified its existence on the grounds of having to defeat the machinations of Brazilian communism—a movement, Leacock demonstrates, of almost no political or military consequence.

This study includes an informative and insightful discussion of Brazilian politics from 1961 to 1965. Organized labor, the military, labor unions, provincial politicians, business, and landowners compete for the limited fruits of Brazilian life sometimes ruthlessly, sometime idealistically, always colorfully. Leacock correctly portrays Perónism as a noncommunist means for the political exploitation of Latin American labor and explains the reasons for its failure in Brazil. Her treatment of the dynamics of politics and foreign policy making in the United States is less sophisticated. Her portrayal of the Kennedy and Johnson administrations as knee-jerk anticommunists seems strained at times. On seeing photographs of missile sites in Cuba in the fall of 1962, Leacock asserts, Kennedy "announced the development as a crisis and seized the opportunity to confront the Soviet Union" (p. 29). However belligerent Kennedy became during the missile crisis, few would argue that he saw it as an opportunity. Nevertheless, this book is well researched and well written, a valuable contribution to the literature on U.S.-Latin American relations.

RANDALL B. WOODS
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ERIC L. MILLS. *Biological Oceanography: An Early History, 1870-1960*. Ithaca: Cornell University Press. 1989. Pp. xvii, 378. \$47.95.

Histories of ecology published during the 1970s and 1980s are essentially histories of terrestrial ecology. Yet also published during the same period were important studies on the history of limnology and marine ecology. Eric L. Mills's monograph is the latest, and perhaps the best, of the latter. He is an oceanographer who is also trained in the history of modern science.

The title of the book is somewhat broader than its actual scope. It is mostly a history of the study of plankton dynamics, undertaken first at the University of Kiel from 1870 to the 1920s, then continued at Plymouth Laboratory and elsewhere in England from the 1920s to 1960.

Mills justifies his title by arguing that plankton dynamics was, from 1870 until 1960, the major con-

cern of biological oceanographers. In 1870, and for decades afterward, the prime motivation for such studies was a decline or drastic fluctuation in the commercial fish harvest in the North and Baltic seas. To manage fisheries adequately, biologists needed to know how the marine ecosystem worked. The food chain that sustains commercially harvested species begins with phytoplankton.

The research story starts with a medical student from Schleswig, Victor Hensen, whose political loyalties are indicated by his residence in a Berlin school for a time to evade the Danish draft. When Schleswig-Holstein was taken by Prussia in 1867, its citizens with interests in fisheries sought state intervention in their declining industry. Hensen, who became a professor of physiology at the University of Kiel, devoted a third of his publications (and career) to marine biology. He wanted to measure the productivity of marine environments but found it difficult to get beyond controversies over the reliability of his techniques for sampling plankton abundance.

His successor, Karl Brandt, spent much of his career trying to demonstrate a similarity between the nitrogen cycles of the oceans and land, where nitrogen-fixing bacteria within plant roots play a prominent role. Although Brandt failed to do so, the scientific school that he and Hensen ran developed the methods and agenda for a new science.

Germany could not fund all of the research, however. Other countries around the North and Baltic seas were also concerned about declining fish stocks, and they jointly established the International Council for Exploration of the Sea in 1902. There is an interesting contrast between the increasing international cooperation of scientists and the increasing political polarization of countries in the dozen years before World War I.

It was not so much World War I, Mills argues, as the rigidity of Germany's university system that led to the decline of the Kiel plankton school. Fortunately, its methods and general outlook were absorbed by the Marine Biological Association, which opened its Plymouth Laboratory in 1888. In the 1920s this laboratory, under E. J. Allen's direction, assumed world leadership in plankton studies.

In the early 1940s, when that leadership passed to Gordon A. Riley at Yale University, it was not because the Plymouth Laboratory had gone into decline, as Kiel had, but because it had gone on to other investigations. A quantitative approach passed from Kiel to Plymouth to Yale, and by 1960 an increasingly complex plankton theory encompassed physical, chemical, and biological factors to explain such phenomena as spring blooms. All of these studies were an essential foundation for modern investigations, which focus more and more on the impact of pollutants and overfishing on the marine ecosystem.

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ANCIENT

DAVID STOCKTON. *The Classical Athenian Democracy*. New York: Oxford University Press. 1990. Pp. 201. \$45.00.

David Stockton's book is aimed both at students of ancient history and at the educated and interested public. It explores democracy in Athens in the fifth and fourth centuries B.C.: the institutions, how they functioned, and what assumptions underlay them. Directing a book toward a double audience fails much more often than not; here it not only succeeds beautifully but even provides both audiences with valuable insights and viewpoints not quite like those available from any other source.

Chapter 1 provides statistical and background information: geographical scale, facts about wealth, currency and land, tax and government finance, and population. All of the accounts are standard and clear with thoughtful attention to supplying comparisons comprehensible to readers on both sides of the Atlantic.

Chapter 2 outlines the development of democracy from the early days of Solon to its full flowering in the mid-fifth century. Chapter 3 describes the workings of the democracy from the bottom up. Chapter 4 looks at the personalities involved in Athenian government. Chapter 5 gives rather full accounts of the two antidemocratic revolutions in the late fifth century. Finally, chapter 6 considers the contemporary attitudes toward democracy of those major figures whose attitudes have survived: Herodotus, pseudo-Xenophon, Thucydides, Pericles (indirectly), Aristophanes, Plato, and Aristotle.

For classical scholars the most interesting material is found in chapter 2. Here Stockton gives an original and persuasive explanation of the effects of (and possibly the motivation for) Cleisthenes's complicated scheme through which each of the ten Athenian tribes drew its members from three geographically separated regions. Since each tribe was put in charge of the city council (boule) for one month of the year, the fact that each tribe consisted of a comparable geographic mixture prevented regional interests from producing wild swings from month to month in the operation of the council. In this chapter Stockton also provides a lengthy and brilliant discussion of the peculiar institution of ostracism. He helpfully discusses a number of problems related to this practice and moves to the excellent conclusion that it could operate much like our modern general elections: in choosing to oust one of two leaders with divergent views, the Athenians could more or less clear the path for some large and general policy over a period of years.

Stockton doubts that there were essential changes in the government after 403 B.C., perhaps underestimating the importance of that crucial divide. But he recognizes that the Thirty Tyrants ultimately strengthened the Athenian love of democracy. His

treatment of politics and politicians lacks the focus of other chapters, but this partly stems from the nature of the topic.

A consistent strength of Stockton's exposition is his continual focus on the effects of the institutions that he describes. Repeatedly he manages to pinpoint critical ways in which the Athenian model intensified individual participation and ensured a sense of identification between citizen and government rather than allowing a sense of "us" and "them." This book can be read quite quickly, but it leaves a lasting impression of the success of this form of democracy and the ways in which it was achieved.

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YVON GARLAN. *Guerre et économie en Grèce ancienne*. (Textes à l'appui.) Paris: Editions la Découverte. 1989. Pp. 225. 98 fr.

The historical writers of ancient Greece discuss wars at great length. Modern writers of ancient military history also discuss wars, but, as Yvon Garlan points out (p. 19), their subject is invariably specific wars rather than warfare in general. Neither the economic structures of Greek warfare nor the Greeks' perceptions of collective violence are well understood. Garlan identifies what we might call a new ancient military history, owing much of its impetus to external factors, particularly changes forced onto Western military thought by wars of decolonization and the possibility of nuclear holocaust (p. 18).

Garlan traces the new military history to the totalizing interests of the *Annales* school, but his book is less a coherent argument relating long-term economic structures to short-term military events than a collection of nine varied studies. Garlan frequently cites with approval Moses Finley's call for an explanation of the role of war and empire in ancient society (*Ancient History: Evidence and Models* [1985], pp. 67–87), but anyone expecting to find such an analysis will be disappointed. Five of the chapters are papers that were previously published between 1968 and 1987, and they cover the distribution of expenses and booty between allies, the enslavement of war captives, the military and social functions of fortifications, and piracy. All are enlightening and, particularly in the case of the studies of fortifications, have stimulated important research by other historians, which Garlan takes account of in these updated versions. The other four chapters are new studies. Perhaps the most interesting is chapter 1, which examines the contradiction between the general explanations for warfare given by Plato (competition for territory, riches, and slaves) and Aristotle (the process whereby those who deserve to be dominated or enslaved suffer this fate) and the different explanations given for specific wars by Thucydides and Polybius that focus on more narrowly "political" factors. As Garlan notes, when

Plato and Aristotle happen to mention the causes of particular conflicts, they, too, abandon their general perception in favor of political accidents (pp. 35–36). Garlan explicitly takes a Marxist position throughout the book and in this chapter resolves the problem by appeal to a broader, Gramscian concept of "the economic" (p. 40). Chapter 4 follows up this idea in detail by demonstrating the infrequency with which Greek wars (unlike Roman wars) led to mass enslavements. The other three new chapters take different topics: the links between war and the spread of coinage, the political dimensions of the rise of mercenary armies in the fourth century B.C., and Karl Marx's and Friedrich Engels's scattered writings on ancient warfare.

Taken together, these studies make many valuable contributions to our understanding of the economic aspects of Greek warfare, but the grand synthetic approach that Garlan proclaims (pp. 18–20) still remains to be developed.

IAN MORRIS
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N. G. L. HAMMOND and F. W. WALBANK. *A History of Macedonia*. Volume 3, 336–167 B.C. New York: Clarendon Press of Oxford University Press. 1988. Pp. xxx, 654. \$124.00.

This is the third and last volume of N. G. L. Hammond's monumental history of Macedonia, written with the collaboration of F. W. Walbank, who contributed the section from the battle of Ipsus to the end of Cassander's dynasty (301–221 B.C.).

This work fulfills a genuine need in Hellenistic history. There are works devoted to Ptolemaic Egypt, Seleucid Syria, Bactria and India, Pergamon, and even small states such as Rhodes and cities such as Athens. The history of Hellenistic Macedonia has been comparatively neglected. Although we have accounts of some individual reigns and sections concerning Macedonia in larger works, there has been no single volume devoted to the country for the whole of the Hellenistic era. Ironically, Macedonia can be said to have initiated the era through the achievements of its king, Alexander the Great. Hammond and Walbank have admirably fulfilled this need and have restored the country to its proper place as an integral part of the Hellenistic world. One of the chief merits of the book is the way it unites scattered and sometimes inaccessible research into a single, well-produced volume.

Before each major section, there is a useful discussion of our scattered and imperfect ancient evidence. The discussions are usually judicious, with only a few lapses. Many will be surprised to find Aristobulus mentioned as one of the "indisputably . . . best authorities" (p. 28) for Alexander's reign, since he was known as a flatterer, even in antiquity.

The book begins with the assassination of Philip II

in 336 B.C., and Hammond exonerates Alexander and Olympias of his death as well as the deaths of Philip's new wife, Cleopatra, and infant son. Hammond believes that the wife and son were executed by orders of the "Assembly of the Macedones sitting as a people's court" (p. 10) because they were relatives of the traitor Attalus I. It is not easy to understand how an infant son of Philip could be thought to be a member of Attalus's family and not his own.

Hammond regards Alexander's destruction of Thebes in 335 B.C. as justifiable and supported by the Council of the Greeks. Arrian records, however, that Alexander entrusted the decision on Thebes to the allies who participated in the attack, especially the Phocians, Plataeans, and other Boeotians, noted for their anti-Theban sentiment. Even the apologetic Arrian notes that the rest of the Greeks regarded the destruction with great horror.

The vicissitudes that the country suffered after the death of Alexander are ably treated. From the greatest world power during his reign, the kingdom disintegrated between 285 B.C. and 276 B.C. and almost became extinct. Macedon's revival during the mid-third century was because of an able line of Antigonid rulers begun by Antigonos Gonatas (276–239 B.C.).

The treatment of Roman intervention into Macedonia and the Hellenistic world is admirable. It is of great benefit to have this story told from the Macedonian perspective for a change. Especially important are the descriptions of the battles of Cynocephalae (197 B.C.) and Pydna (168 B.C.), where the Macedonian phalanx challenged the Roman manipular legion. Both battlefields receive personal autopsy by Hammond; therefore, the descriptions make an original contribution to the military history of the era. Although the Romans were not able to defend themselves against the frontal assault of the phalanx, the manipular formations were flexible enough to attack from the rear (in the case of Cynocephalae) and to break the phalanx line once it had become disorganized through a too rapid pursuit (Pydna).

Hammond notes that the terroristic methods of conquest and rule the Romans employed against Macedon and Epirus, where 150,000 innocent civilians were sold as slaves in 167 B.C., induced poverty, apathy, and ultimately desperate resistance that was to manifest itself in 146 B.C. and later in 88 B.C. From the time of Alexander, however, the Macedonians had sought collaboration with the conquered peoples and allowed them to continue to make cultural contributions. Not until the Principate did the Romans begin to collaborate with the peoples of their empire, which renewed the cultural life of the eastern half of the Roman world.

One would have hoped for a brief section or appendix on the country's history from 167 B.C. to 146 B.C., when it finally lost its independence. There are still interesting stories to tell about the effects of the Roman settlement of 167 B.C. and the resistance of Macedonia's last king, Andriscus. It would also

have been useful to have devoted a section or two to social, economic, and cultural matters.

These are minor cavils for a work of indispensable value for all Hellenistic historians and students of Roman imperialism.

DONALD ENGELS

University of Arkansas

RICHARD A. BILLOWS. *Antigonos the One-Eyed and the Creation of the Hellenistic State*. (Hellenistic Culture and Society, number 4.) Berkeley and Los Angeles: University of California Press. 1990. Pp. xix, 515. \$55.00.

The extraordinary life of Alexander the Great caught his contemporaries by surprise, and so did his death. Leaving behind no obvious or able successor, Alexander unleashed on his empire the ambitions and jealousies of his own generals. The first of these marshals to be made a king was Antigonos Monophthalmus, a one-eyed giant from Macedonia who towered over the tumultuous years of the early Hellenistic period until his death in battle at the age of eighty-one. Capturing all of the drama of Antigonos's career, while questioning much in modern scholarship, Richard A. Billows has crafted a biography destined to become a standard reference in the field. Originally a dissertation in 1985, the book covers the life of Antigonos in five chapters, followed by three chapters on Antigonos's relations with the Greeks, the administration of his Asian realm, and the king's policies regarding colonization, cultural patronage, and the economy. The indexes, maps, charts, bibliography, and three appendixes contribute as much to the usefulness of this volume as the text itself.

Billows has overcome problems associated with Hellenistic biography such as sparse evidence and moralizing ancient judgments, in addition to the tendency of every biographer to admire and exaggerate the character and achievement of his subject. Only at the extremes of Antigonos's life (chapters 1 and 5 on the early and final years) does the narrative falter, the lack of evidence giving rise here to some rather bold speculations. "Nothing specific is known of his youth," admits Billows (p. 18), yet Antigonos emerges from the shadows a heroic and prominent aristocrat solely because of the author's favorable interpretation of a few disputed events and anecdotes. For example, even if Antigonos did lose his eye fighting at Perinthus (although Plutarch says it was someone else), the circumstances hardly support the further suggestion that Antigonos held a key command during this campaign (pp. 27–29).

With the better documentation available for Antigonos's role in the struggle of the Diadochi ("successors" of Alexander), the narrative gathers momentum and sweeps masterfully through one of the most complex periods in ancient history. The judgments here are sensible. Billows follows Diodorus closely but complements and occasionally corrects this main

source with a wide range of epigraphic and other evidence. With the possible exception of Antigonos's own son Demetrius, Billows treats the great allies and enemies of his subject fairly. In chapter 5 the tendency to blame Demetrius for Antigonos's later failures will not likely go unchallenged. The final three chapters neatly summarize the impact of Antigonos's reign on the state that emerged from his defeat, particularly the empire of the Seleucids. Billows concludes that Antigonos was an influential strategist and state-builder whose reputation has suffered unfairly from the final disaster at Ipsus that ended his life in 301 B.C.

This book is an important reassessment of Antigonos that makes the man less vain and ruthless but also less visionary. No dreamer cast in the mold of Alexander, Antigonos appears in these pages as a determined realist who looked back to Philip and even to the Achaemenids as he led the way into the Hellenistic age.

FRANK L. HOLT
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DEMOSTHENES. *Against Meidias, Oration 21*. Edited and translated by DOUGLAS M. MACDOWELL. New York: Clarendon Press of Oxford University Press. 1990. Pp. xvi, 440. \$98.00.

In 348 B.C. Meidias punched Demosthenes in the face with his fist. The orator immortalized the incident in a masterful speech in which he sued his assailant. This edition by Douglas M. MacDowell, an eminent authority on Athenian law, is based on a collation of more manuscripts than previous editions. The introduction expounds the textual tradition, the contestants' long feud in and out of court culminating in Meidias's attack during Dionysus's festival, the methods of prosecution and defense, and the problems of composition and delivery.

Although they had been occasional political opponents, Demosthenes's motive for suing Meidias was personal: the two hated each other.

The prosecution took the form of a *probola*, the "putting forward" of a charge before the assembly, here, "committing an offense during a festival." The assembly decided whether a case should go to court because officials empowered to do so did not exist. Despite modern opinion, *probola* also comprised the ensuing jury trials to reach a verdict and assess the penalty.

Demosthenes also accused Meidias of impiety and insolence (*hubris*), although not formally in the indictment. Lacking statutory definitions, the meaning of these terms was determined by the jury. *Hubris* was an offense because it dishonored the victim of assault and demonstrated the attacker's equally culpable attitude of self-indulgence and indiscipline.

Compositional defects suggesting that the speech was not revised and Demosthenes's acceptance of

money from Meidias after winning in the assembly have been interpreted to mean that the orator did not go to court and never gave the speech. The structural weaknesses, however, are overstated; determined to uphold the rule of law, Demosthenes was not likely to accept a bribe to drop the case. The money changing hands was the fine imposed on Meidias by the court, where Demosthenes also succeeded. The present speech is a draft from which Demosthenes extemporized in court.

By cleverly mixing arguments, some of which would be inadmissible today, with the more exciting narrative of the quarrel, which blackens Meidias's character, Demosthenes sustained the jurors' interest and made them feel an injured party, too.

The commentary is a model of its kind. Adducing previous research and new inscriptional and archaeological evidence, MacDowell addresses most problems of the speech. As one might expect, he is best on laws, litigation, and courtroom procedure. His comments on such matters as public and private suits, countersuits, premeditation, arbitration, penalties, witnesses, and much more advance our knowledge or correct misconceptions. MacDowell is also excellent on constitutional and institutional points and on religious customs, but he is less successful on some topics outside his expertise. On crowns, he could have profited from Michael Blech's *Studien zum Kranz bei den Griechen* (1982). His comments on naval matters sometimes miss the mark completely. More regrettable is his tendency to misquote other scholars and then argue against misunderstood views.

The translation facing the text is accurate and very readable but makes the book unsuitable for classroom use. An *index locorum* would have been a useful addition to the indexes of subjects and Greek words.

BORIMIR JORDAN
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JOHN J. WINKLER. *The Constraints of Desire: The Anthropology of Sex and Gender in Ancient Greece*. New York: Routledge. 1990. Pp. x, 269. Cloth \$47.50, paper \$14.95.

Like his mentor, the French philosopher Michel Foucault, John J. Winkler died of AIDS, leaving behind a manuscript on the sexual mores of the classical Greeks. Unlike Foucault, who for all his earlier brilliance revealed but a shallow grasp of Greek culture (see my review of his *Use of Pleasure* [1984] in the *Philadelphia Enquirer*, January 26, 1986), Winkler was an accomplished classicist and, also unlike his model, one with feminist leanings. This volume is a collection of essays, organized under the rubrics "Men" and "Women," of which only one essay (chap. 2, under "Men") deals directly with sexual behavioral norms. Here Winkler points to the Athenian paradigms of ideal and reprehensible manhood,

namely, the hoplite (heavily armed infantryman) and the *kinaidos*, which is usually translated as "catamite" (a word derived from the name of Ganymede, Zeus's boy lover). Winkler proffers little comment on the hoplite as a symbol of righteous virility (Kenneth Dover, in his excellent *Greek Homosexuality* [1978], points to the financial component in this ideal inasmuch as the hoplite had to provide his own armor, and as Lysias 14, *Against Alcibiades*, on a charge of desertion, makes clear, infantry duty was considered more hazardous than that of the cavalry). With regard to the catamite as the prevalent symbol of depraved masculinity, Winkler does not fulfill his promise to give an exact definition of this type, nor does he tackle the fundamental conundrum of Greek sexual morality: if pederastic homoerotic relationships in Greece were institutionalized and endowed with parental values, why were there diatribes against the catamite and Draconian measures against the "male prostitute"? Where exactly was the dividing line between the legitimate role of a boy who grants sexual favors in exchange for social ones and material gifts and the "male prostitute" (who stood to be deprived by law of his civil rights)? And what exactly are the "terrible and lawless" homosexual acts of Plato, *Phaidr.* 254 A, and the "shameful lusts" of Aeschines, *Tim.* 160? In both cases the reference is to relationships between adult and youthful males, which appear to fall inside the parameters of canonized behavior. Winkler does not resolve these paradoxes for us. (Whatever the exact distinctions are, the adoption of a passive stance, as epitomized by submission to anal sex, is at least part of the definition of the *kinaidos*.)

An opening chapter attempts to deconstruct an essay on the interpretation of dreams from the late second century A.D. by Artemidorus Daldanus. Winkler comes to the apt conclusion that, in the perennial Greek antithesis of *phusis* versus *nomos* (nature versus law or culture), "nature" is really what "culture" defines it to be, but the relevance of this finding to classical civilization is dubious. An essay on the (mostly postclassical) magical texts in Greek notes the sadistic element often present.

The remainder of the book consists essentially of literary essays, for which Winkler dons his feminist hat. "The Education of Chloe" presents the sexual initiation of the heroine of the Hellenist novel *Daphnis and Chloe* as an introduction to human violence. In "Penelope's Cunning and Homer's," Winkler argues that Homer portrayed Odysseus and his wife as equals, overlooking the fact that the poet upholds the double standard of sexual morality when he makes the notoriously unfaithful Odysseus react with anger to the suspicion of adultery on the part of Penelope (*Od.* 23, 181). In two final chapters Winkler searches for countercultural activities on the part of women "behind the facade of public docility" (p. 209). In the case of the poetess Sappho, the author has no argument. Most readers of the meager and corrupt frag-

ments agree that they express homoerotic feelings, but nothing indicates that these were branded as reprehensible in Sappho's own environment. Moreover, the poetess earned special praise in antiquity for her wedding hymns. On the other hand, countercultural elements in women's rituals at Athens are unmistakable. The author cites the rites of Demeter and Adonis. For the cult of Demeter and Persephone, he overlooks his best evidence, namely, the (unsuccessful) introduction of an artificial male divinity, Iakchos/Triptolemos, into the ritual in order to defeat its feminist aspects. The myth and ritual of Adonis abound in antipatriarchal elements, but these have been well established for some time now. The best passages in this collection are those that offer literary exegesis, and one can only regret that the untimely death of this original and compassionate scholar prevented him from further refining them.

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DIETER FLACH. *Römische Agrargeschichte*. (Handbuch der Altertumswissenschaft, number III, 9). Munich: C. H. Beck. 1990. Pp. xii, 347. DM 145.

Dieter Flach's work consists of six chapters of unequal length. The first briefly examines the methods and tools employed by the Roman surveyor and the historical and legal devolution of his profession. The long second chapter examines the history of agrarian legislation in the second and first centuries B.C.; the third deploys epigraphic evidence to explore the tenancy agreements used on imperial estates in North Africa during the second century A.D. The fourth and fifth chapters respectively take up the principles of estate management and the physical organization of large estates. The concluding chapter briefly exhumes what the major agricultural writers tell us about a variety of capital intensive agricultural and pastoral enterprises such as viticulture, sheep ranching, and poultry management.

Because the study is part of an encyclopedic series, it would be unreasonable to expect bold new insights from this volume. One must ask instead whether it faithfully reflects the current state of knowledge in the field. For this purpose it may be usefully compared with K. D. White's *Roman Farming* (1970), another encyclopedic work that is now twenty years old. It will be instantly apparent that both authors view Roman agriculture from the narrow perspective of the elite. They are both slavishly dependent on Cato, Varro, and Columella; hence, it is hardly surprising that Flach introduces archaeological evidence to test their recommendations for the physical layout of an estate rather than to explore features of the rural landscape that they ignore (pp. 215–49). White and Flach share, in short, a common perspective and differ only in the emphasis that they assign to given

topics. White, for example, discourses at length on soil management and virtually neglects tenancy arrangements; Flach devotes fully one-third of his book (chapters 3 and 4) to the latter subject and wholly ignores the former.

With its elite perspective and emphasis on literary sources, White's book typifies the methodological approach to Roman agriculture in 1970. Flach, however, virtually neglects twenty years of ensuing research on the social history of the Roman countryside that has fundamentally altered both our perception of rural life and approaches to it. Thus, he offers only a few scattered allusions to the female overseer on a large estate (on whom see W. Scheidel, "Feldarbeit von Frauen in der antiken Landwirtschaft," *Gymnasium*, 97 [1990]: 405–31). As K. R. Bradley has shown in a pair of recent books (*Slaves and Masters in the Roman Empire*; *Slavery and Rebellion in the Roman World*), one can also give rural slavery a human face—but only if one makes a conscious effort to abandon the traditional vantage point of the elite. Finally, although the peasant economy has been intensively studied in the last two decades, Flach relegates the peasantry to three uninformative pages (pp. 123–25). This occasions surprise, for he clearly understands the symbiotic relationship that existed between large landowners and neighboring *rustici* (p. 83).

This is, then, a disappointing effort. A synthesis that welcomes the innovative approaches to the Roman countryside that have emerged over the past twenty years and incorporates their findings remains a major desideratum in Roman history.

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RICHARD A. BAUMAN. *Lawyers and Politics in the Early Roman Empire: A Study of Relations between the Roman Jurists and the Emperors from Augustus to Hadrian*. (Münchener Beiträge zur Papyrusforschung und antiken Rechtsgeschichte, number 82.) Munich: C. H. Beck. 1989. Pp. xxxi, 336. DM 98.

This book is written for specialists in the history of Roman law. It has nothing to do with the law itself but rather with its external features, in particular with the *iurisconsulti*, experts in law, and their schools in the first century and a half of the Roman empire, their attitudes toward the emperors, and their different approaches to certain aspects of the law. The transition from republic to empire necessarily meant a shift of authority and a different philosophical change in the profession of law.

Richard A. Bauman takes much for granted in his readers, the most important of which is the knowledge that there was a distinct difference between lawyers and orators in ancient Rome. The famous Cicero, for example, was not a lawyer. He was an

orator, a specialist in forensic speechmaking, and accordingly knew little of the law. Like his colleagues, he thought himself superior to the *iurisconsulti*. As an orator he would consult a *iurisconsultus* to find out what law pertained to his particular case, and then he would prepare his speech for delivery in the court. These *iurisconsulti* were legal scholars: senators and magistrates who conducted private and public consultations, drafted legal documents, and gave advice on the proper procedure in the courts. In previous works, Bauman had discussed these men and their careers during the republic, and in the present book he carries their history into the empire.

The right to give legal opinion to the public at large was naturally a vital concern to the first emperor, Augustus, and he sought to ally the public to his cause by granting such a right to some of them with the stamp of his own authority (*ex auctoritate principis*). Under Augustus there arose two schools of law, and legal education for the first time now assumed a more organized and academic form. One school was apparently founded by Ateius Capito, known later as the Sabinians, and a second by Antistius Labeo, known later as the Proculians. Bauman traces the history of these two schools and examines all of the modern attempts to explain their nature. He concludes that they were teaching schools, each of them differing from the other on points of law and imperial legislation. Because of these schools a certain competence or standard was expected of future *iurisconsulti*. The profession of law was licensed. The golden age of the Proculians came under the emperor Trajan when Neratius Priscus, head of the Proculian school, came under consideration to become the next emperor. Trajan had regularly turned to the Proculians rather than to the Sabinians for legal advice, a situation that must have existed in the past as well and must have often contributed to the hostility between the two schools.

In addition to the history of these schools and their members, Bauman also gives a thorough investigation of the public careers of the *iurisconsulti* and their attempt to acquire a special status of their own "adapted to their special skills." As he briefly puts it: "They persuaded the emperors to restructure the *ius respondendi* and the *consilium principis* to give them systematic entry into the senate, and to open wide the doors of the imperial service" (p. 320). Thus, like their counterparts in the military and the provincial administration, the jurists could now find their place in an official capacity. They finally "abandoned ideological opposition to the Principate" (p. 321), that is, to the empire, as more and more of them reached the consulship. The best of them sat on the emperor's *consilium*, his standing committee for legal affairs, and thus were always close to the throne and the power that it represented.

Bauman's book is closely reasoned and will be of interest largely to specialists in Roman law, but it is also clear that the political historian will find much of

interest because of the special nature of the *irriscon-sulti* and their proximity to central power. On all of these points, this book will long remain both a summary of previous opinion and an expression of Bauman's own solutions to a host of small and large problems.

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BARBARA LEVICK. *Claudius*. New Haven: Yale University Press. 1990. Pp. xvi, 256. \$25.00.

The emperor Claudius had a hard time. Afflicted from infancy with physical handicaps and thought to be feeble-minded, he was systematically excluded from the honors due an imperial prince. When he unexpectedly became emperor at the murder of his nephew Caligula, the ancient sources presented him as the foolish tool of his profligate wives and calculating freedmen. Even death by poisoned mushrooms and his ensuing deification did not win him a reprieve. Seneca's biting satire shows Claudius as a new "god" turned away from Olympus and doomed to everlasting service in the underworld as a law clerk.

Early in this century, newly discovered papyri and inscriptions revealed Claudius to be an effective and humane administrator, and rehabilitation of the emperor began. Arnaldo Momigliano (1932) and Vincent Scramuzza (1940) showed that the archaeological and nonliterary sources provide an image of Claudius the administrator and Claudius the builder that is inconsistent with the buffoon portrayed by Suetonius and Tacitus. This rehabilitation, perhaps best known through the novels of the 1930s by Robert Graves, became the new gospel.

Barbara Levick provides a new treatment of the reign of Claudius; it is a biography and much more. His reign was the turning point in the establishment of the permanent monarchy, and Levick is excellent in showing how Claudius worked throughout his reign to secure his position. His cruelty toward senators, his generosity toward provincials, his attempts to rally the support of the Roman masses, his fatal marriage to his niece Agrippina, and even his successful invasion of Britain must all be seen as part of a political struggle for personal and dynastic survival.

Although Levick at times stakes out a position critical of Momigliano, as when she criticizes his idea of centralization as an anachronism from "the heyday of the Dictators" (p. 81), her book actually broadens and deepens his rehabilitation of Claudius as a remarkable administrator. She doubts that Claudius developed a "civil service" and asserts that he did not have a financial policy, but her detailed discussions of these topics undercut her assertions. For example, Levick admits that Claudius understood Rome's financial problems and tried to meet them by "economy and management" as well as foreign conquest. If

he did not spell out a financial policy, his practical performance confirms a grasp good enough by ancient (or modern) standards to be called "policy."

The book might appear to the browser to be a popular biography. It is nothing of the sort. The notes—too densely presented for the casual reader—show Levick's acquaintance with recent scholarship not only on Claudius but on a wide range of problems in the early empire. The author provides insight on a wide range of topics: the jealousy of Italian senators toward the immensely wealthy Gaul Asiaticus; the repeated political exploitation of the British conquest despite its long-term economic and military cost; and Claudius in the role of the "last of the populares." This stimulating book is the best we have on Claudius and among the best current treatments of any Roman emperor.

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JOHN MATTHEWS. *The Roman Empire of Ammianus*. Baltimore: Johns Hopkins University Press. 1989. Pp. xiv, 608. \$55.00.

Edward Thompson began his slim classic study, *The Historical Work of Ammianus Marcellinus* (1947), with the pessimistic observation that "for every reader of his work nowadays there are a thousand of Sallust, Livy or Tacitus." Modern interests in the ancient world have changed greatly over the last forty years, and Ammianus is gaining still more readers through the recent Penguin translation of the greater part of his history. John Matthews, too, originally envisaged a slim volume on Ammianus, but the richness of the historical material has fortunately persuaded him to compose a large and rich book whose historiographical model is not Thompson's work but Ronald Syme's *Tacitus* (1958), a classic of a very different order.

Ammianus wrote a history of the Roman empire from 96 to 378. The surviving portion commences with the events of 353, contains a full and continuous narrative to the middle of 364, and then provides a more discursive account of the reigns of the emperors Valentinian in the West (to 375) and Valens in the East (to 378), concluding with the Battle of Adrianople and its immediate aftermath. The first part of Matthews's study, entitled "Res Gestae" (pp. 3–228), takes the reader through Ammianus's narrative chronologically, though with some thematic excursus, such as a chapter called "Julian and the Philosophers." The second part, entitled "Visa vel Lecta" (pp. 231–472), illustrates and discusses the material presented by Ammianus in eight chapters organized and linked together thematically. Matthews's survey of the Roman empire as seen in the pages of Ammianus starts from the emperor and the functioning of civilian government. Because the central role of the emperor in the fourth century was, as Matthews

correctly emphasizes, that of supreme commander of the empire's military forces, consideration of the emperors' military activities provides a natural transition to the nature of warfare, which in turn leads to Rome's enemies, classified as "bandits and brigands" or, alternatively, as farmers, nomads, and rebels. Next come the physical environment of town and country and social relationships. The book concludes with religion and philosophy, and the culture of Ammianus himself—the Greek who wrote a Latin that describes scenes so vividly and so graphically that most readers find Ammianus's images fixed forever in their minds. The endnotes, which, together with a very full bibliography and an excellent index, occupy more than one-fifth of the volume (pp. 473–608), provide the best available introduction to modern scholarship on Ammianus. Matthews shows himself a reliable guide to what has been achieved and what still remains problematic.

The greatest strengths of the book are its sense of place and occasion and its author's success in enlarging the cultural background against which Ammianus will in the future be read. Both virtues are immediately displayed at the outset. The introduction opens with a brilliant evocation of the Syriac poet Ephrem looking down from the walls of Nisibis on the defeated and despondent Roman army, which was bringing the wounded body of the emperor Julian back from Persia for burial. And the longest and most illuminating chapter is the one that depicts the different geographical settings and the varied cultural and political contexts in which Ammianus wrote about Alamanni and Goths, Huns and Saracens, Isaurians and Moors. Matthews has succeeded magnificently in interpreting the history of Ammianus for a wide modern audience in all its fullness of detail.

The great virtues of the book are regrettably not enhanced by its style. Matthews writes in a leisurely manner that too often verges on the verbose or lacks the sharpness of the historian about whom he is writing. Moreover, one central feature of the book will strike many specialists as very traditional. Like Edward Gibbon, Matthews regards Ammianus as "an accurate and faithful guide" to the historical period about which he wrote: "it will be obvious that he is a wonderfully eloquent witness of almost every aspect of the life and society of his times" (p. 228). Yet there are many places where Matthews is compelled to adopt a less favorable estimate of the quality of Ammianus's historical insight: on the tensions between the emperors Gallus and Julian and the city of Antioch, for example, he concludes that Ammianus could not "engage the question of the food supply in its true dimension, as a problem of economics, social organisation and government" (p. 409).

Matthews has given us a superb summation of a method of interpreting Ammianus that, especially in his hands, has been productive and illuminating. But

future progress may well require a different, more analytical approach to his history.

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ALINE ROUSSELLE. *Croire et Guérir: La Foi en Gaule dans l'antiquité tardive*. Paris: Fayard. 1990. Pp. 382. 140 fr.

In this fine discussion of the spread and impact of Christianity in late antique Gaul, Aline Rousselle begins by taking paganism seriously. Her first concern is to separate the destruction of pagan shrines from Christian hostility and intolerance. Arguing primarily from archaeological and numismatic evidence, she instead associates the decline of some (but certainly not all) sanctuaries during the later third and fourth centuries with the impact of the barbarian invasions that left cities unable to support these pagan cults financially. Rousselle then discusses the medical handbooks of Oribasius, a Greek who joined the emperor Julian in Gaul, and Marcellus, a Gallic senator who collected the recipes for hundreds of remedies and potions. Because people employed similar techniques at shrines constructed at fountains and hot springs, Rousselle emphasizes well the implications of the appropriate rituals that first offered people an explanation for their ailments and then provided them with the opportunity of direct participation in their own therapy.

Rousselle next discusses the significance of Saint Martin, the wonder-working bishop of Tours from 371 to 397. In her view, Martin's reputation as a healer represented a fundamental shift in people's thinking, "the substitution of the man for a place" (p. 130). This section of her book is largely a restatement of an argument first published in *Annales*, 31 (1976) and now available in translation in *Ritual, Religion, and the Sacred*, edited by R. Forster and O. Ranum (1982), and it again stresses the "therapeutic relationship" that developed between Martin and sick people. The subsequent cult of Saint Martin retained these personal relationships, even as it also restored the emphasis on sacred places (now tombs and churches) and sacred objects (now relics). Rousselle can even speculate rather ingeniously on the replacement of pagan shrines at fountains and springs with Christian baptisteries.

Rousselle's insights are wonderfully subtle and suggestive, even if sometimes a bit enigmatic. Some of her discussions are outstanding, in particular of the roles of Sulpicius Severus and Paulinus of Nola in the promotion of saints' cults and of the philosophical logic underlying the treatise by Victricius of Rouen about saints' relics. Rousselle rightly rejects any attempt to associate saints' cults with pagan survivals or popular religion; she also opposes the currently fashionable "political model" that makes clerical elites responsible for the imposition of relic cults. Yet her own preferred alternative is not satisfying either. In

her discussions of both pagan and Christian healings, Rousselle consistently relies on psychoanalysis for both terminology and explanations. In place of consideration of other consequential transformations, such as the rise of bishops and the decay of the imperial administration, Rousselle prefers to investigate "the psychological circumstances" (p. 52). Not only does she claim that a notion of personal responsibility eventually generated less anxiety than a theory that made God responsible for evil, but she also suggests that the extension of citizenship by the emperor Caracalla in the early third century upset the "normal phantasies" (p. 228) about relationships between fathers and sons. Because she so narrowly defines the society of late antique Gaul in terms of "psychosomatic and mental disorders" (p. 224), her own perspective, for all of its intelligence and good sense, is therefore more diagnostic and therapeutic than analytical.

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MEDIEVAL

IRFAN SHAHĪD. *Byzantium and the Arabs in the Fifth Century*. Washington, D.C.: Dumbarton Oaks. 1989. Pp. xxvii, 592. \$40.00.

After *Rome and the Arabs* (1984) and *Byzantium and the Arabs in the Fourth Century* (1984), Irfan Shahīd's immensely impressive enterprise moves majestically into its penultimate phase, with an examination in this present volume of the Arabs in the fifth century A.D., their relations with Byzantium as *fæderati*, and their contacts with and absorption of Byzantine Christianity and its culture. The study embraces such basic topics as the extent among the Arabs of writing, their literary productions, and the evolution of the classic Arabic alphabet. A volume on the sixth century will complete this great work.

It is a bold enterprise, in these days of much microscholarship, to embark on a topic with such a grand sweep, for the author must draw on material from such diverse cultural spheres as the late classical, the Christian Oriental, and the pre-Islamic Arabian ones. Shahīd is one of those rare scholars who combines the appropriate linguistic and philological skills with a keen historical insight. He is obviously inspired, moreover, by a (well-justified) mission to correct the widely prevalent view of the early Arabs—or at least, those outside the lands of the ancient South Arabian civilization—as illiterate nomadic barbarians only brought to a culture worthy of the name by the emergence in western Arabia in the early seventh century of the new faith of Islam, and, although he does not expressly say so, one senses that he wishes to refute the view that Arabic culture is to be equated exclusively with Islam.

The book has two main parts, an examination of

the Arabs in *Oriens*, that is, the western end of the Fertile Crescent, with reference also to northwestern Arabia, the Hījāz, and Najrān and South Arabia, first from the Greek and Latin sources (part 1, pp. 3–230) and then from the Arabic ones (part 2, pp. 233–458). In conclusion, there are shorter sections on frontier and *fæderati* studies (part 3, pp. 459–86) and a synthesis and exposition (part 4, pp. 487–539), which bring together the threads of the book.

In the forefront are the Arab *fæderati*, in treaty relationship with Byzantium as its frontier guards, dominated in this century by the tribe of Salīh, or the Zokomids, from whom came the first Salīhīd phy-larch, Du'jum (Zokomos). Shahīd's careful tracing of the Salīhīd ascendancy in *Oriens* (with a base in Moab and the Balqā', where a village, al-Salīhī, and a stream, 'Ayn al-Salīhī, are today the sole surviving toponyms relating to this tribe) elucidates for the first time this obscure aspect of early Arab history, for the Salīh, unlike their *fæderati* predecessors the Tanūkhids and then, after 502, their successors the Ghassānids, virtually vanish as a distinct group after Islam appears, causing them to be neglected and forgotten by post-Islamic Arab writers. Equally valuable is Shahīd's exposition of the extent and strength of Christianity among the Arabs of *Oriens* and northwestern Arabia. He makes the points that the Arab bishops, whose names we know from Greek and Syriac sources, were strongly Orthodox and attached to the Byzantine imperial connection, whereas by the sixth century the Ghassānīd-led *fæderati* had become Monophysite in theology, a change paralleled in the great center of South Arabian Christianity, Najrān, which was originally Orthodox and Nestorian but by the time of the great persecutions and martyrdoms of 520 (on which Shahīd has earlier shed much light by his *Martyrs of Najrān: New Documents* [1971]) had become firmly Monophysite. In regard to this latter region, he suggests Monophysite influences both from the north, the Ghassānids, and from the Ethiopians in the south, coupled with an ascendancy of Monophysitism in Byzantium itself during the late fifth and early sixth centuries (pp. 373–74); these explanations seem very likely. Notable here, too, demonstrates Shahīd, was the early manifestation of an Arab penchant for the vocation of monk, nun, or hermit, a feature of church life along the desert fringes of *Oriens* and Mesopotamia. This penchant was noted by the pre-Islamic and early Arab poets and writers, and descriptions of it blossomed into a minor genre of literature, the *kutub al-diyārāt* or "Books of Monasteries" of 'Abbāsīd times, although by then interest in these monasteries concentrated on them as centers of wine making and as places to which socially minded Muslims could withdraw from the frustrating pietism of much of court life and indulge in a freer style of life.

As an Islamic historian, I have no competence to assess Shahīd's use of non-Arabic sources. But we owe him a great debt of gratitude for tackling a subject

concerning these pre-Islamic Arabic "Dark Ages," bristling as they are with obscurities and *vexatæ quæstiones*.

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FERENC MAKK. *The Árpáds and the Comneni: Political Relations between Hungary and Byzantium in the Twelfth Century*. Budapest: Akadémiai Kiadó. 1989. Pp. 213. \$28.00.

Ferenc Makk, a member of the new generation of Hungarian historians, offers a comprehensive study of an important period of Byzantine history that witnessed the last great revival of the empire under the dynasty of the Comnenian emperors. During this period the relations of the empire were particularly close with the Kingdom of Hungary, which under the dynasty of Árpád had already risen to considerable power in the Danube basin. The relations between the two countries were largely determined by two factors: a close family relationship between the two dynasties and the geopolitical situation of Hungary, the closest neighbor of Byzantium on the northern border along the Danube and the Sava rivers. The close family ties were established by the marriage of the daughter of King Ladislas I of Hungary with John Comnenus, son and successor of the emperor Alexius I. The Hungarian princess, who as empress took the name Irene, was mother of the emperor Manuel. Thus, in the female line, he, as grandson of Ladislas I, was a descendant of the dynasty of Árpád. The family ties were potential sources of conflict, because Byzantium became frequently involved in the internal throne disputes of Hungary, and unsuccessful Hungarian throne pretenders sought protection at the Byzantine court. Another source of conflict was the Hungarian expansion toward Croatia and the Adriatic Sea that started in the late eleventh century under King Ladislas I and was continued by his successor, King Coloman, who occupied the Dalmatian cities on the coast, although Dalmatia nominally remained a Byzantine theme. New hostilities started when Hungary gave assistance to the Serbian independence movement that was led by Uros II, *zupan* of Rascia and vassal of Byzantium. The emperor Manuel retaliated in a series of punitive campaigns by occupying Hungarian territory in the border region of Sirmium. When peace was concluded, Manuel, in order to keep Hungary safely in the Byzantine orbit, invited the younger brother of the Hungarian king, Prince Béla, to the court of Byzantium. Béla was betrothed with Manuel's daughter and designated as heir to the Byzantine throne, foreshadowing a potential personal union between the two countries. Eventually the engagement was broken off when Manuel had a son from his second marriage; nevertheless, Béla remained in Constantinople and married Manuel's sister-in-law. He later returned to Hungary to

occupy the throne and, as King Béla III, remained a loyal friend of Byzantium, even after Manuel's death, until the end of his reign in 1196.

In focusing on the central theme, Byzantine-Hungarian relations, the book does not present much new to the specialist in this particular field. Its true value lies in the fact that the author extends his investigation to the major diplomatic and military activities of other powers that affected Byzantine-Hungarian relations. He presents the full spectrum of twelfth-century politics as background for his study, thereby offering a valuable source of information for the specialist and the interested reader of medieval history.

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J. H. W. G. LIEBESCHUETZ. *Barbarians and Bishops: Army, Church, and State in the Age of Arcadius and Chrysostom*. New York: Clarendon Press of Oxford University Press. 1990. Pp. xiv, 312. \$65.00.

To appreciate the strengths, and the weaknesses, of this book, the reader should begin with the conclusion: The Historian's Post-Mortem. Post-mortem is particularly appropriate, for J. H. W. G. Liebeschuetz performs yet another autopsy on historiography's most venerable cadaver, the Roman empire. His examination of two probable causes of death, demilitarization (also known as "barbarization") and Christianization, is highly professional—with succinct assessments of previous opinions—and relies on contemporary accounts of the final decades of the decedent's decline (pp. 392–412). Liebeschuetz's analysis, however, is unconventional. Unlike most of his predecessors, he assays the Western collapse from an eastern perspective, through events in Constantinople rather than in Rome, and produces the most persuasive explanation to date of the survival of the East.

Confronted with an incessant demand for more troops, and unable to secure recruits from the "plundered, though taxpaying citizens" (p. 53), the Western authorities of the late fourth century relied on barbarian federates to defend the imperial frontiers. This dependence undermined the authority of the civilian government and allowed the commanders of these units, often rewarded with the rank of *magister militum* (for example, Stilicho), to become virtual rulers of the West. In 410 the Western leadership was in such disarray that the Gothic general Alaric could lead his wandering group of displaced federates from Pannonia to Rome without encountering serious military opposition. Although the East could have followed a similar course, the authorities in Constantinople managed to restrain the freewheeling federates they employed. It was the "Gainas affair" in 401, Liebeschuetz argues, that persuaded the Eastern gov-

ernment to use fewer German troops and to place greater reliance on diplomacy. This restraint was possible in the East because there were only two exposed frontier zones, in Mesopotamia and along the Danube. From 392 until 412 this policy was maintained by the "Arcadian establishment," the Eastern Roman aristocracy composed of wealthy citizens from the largest Greek cities who controlled the Senate of Constantinople. Under this civilian management, "the Eastern provinces embarked upon an age of stability and prosperity" (p. 248).

Liebeschuetz's defense of Christianity against the charge that it undermined the response to military threats and contributed to the social and economic disintegration of the late empire is less satisfactory. The lengthy discussions of the fall of John Chrysostom and the career of Synesius of Cyrene (pt. 3) do not offer tangible support for the defense and have little to do with the larger issue of demilitarization. Nor can these events and personalities contribute to an explanation of the survival of the East. Even if the Christian affiliation of the Arcadian establishment were important in some of the ecclesiastical events under scrutiny, the fact remains that Christianity had become the dominant religion in both East and West by the early fifth century.

This misguided effort to refute eighteenth-century charges in no way diminishes Liebeschuetz's contribution to scholarship on the late Roman empire. Adducing the familiar texts, disparate and often tendentious, he has constructed a novel explanation of the survival of the East more persuasive than any of his predecessors. No mean achievement in a tradition inspired by Gibbon.

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SIMON LLOYD. *English Society and the Crusade, 1216–1307*. New York: Clarendon Press of Oxford University Press. 1988. Pp. xiii, 329. \$59.00.

A prominent and welcome feature of recent histories of the Crusades has been a more intensive study of developments that occurred after the fall of Jerusalem to Saladin in 1187 and of Constantinople to the forces of the Fourth Crusade in 1204. The Crusading bulls of Pope Innocent III (1188–1216) and the Crusading canon of his Fourth Lateran Council (1215) made the Crusade a permanent responsibility of the whole of Western Christian society; it was not an occasional activity of mainly the military classes but the daily concern, in one way or another, of all sorts of men and women. In a famous phrase of Sir Maurice Powicke, the recovery of the Holy Land was henceforth inseparable from the air that they breathed. Thus, in the thirteenth century, the Crusade was not only, or even mainly, important for what happened in the eastern Mediterranean or wherever there were military expeditions. If these were surpris-

ingly frequent, they were, especially in the Holy Land itself, often unfruitful; by 1291 the Crusader settlement on the Palestinian and Syrian mainland had come to an inglorious end. Much of the interest of thirteenth-century Crusading lies in its manifestations and consequences in the societies of Western Europe itself. Simon Lloyd's admirably researched and presented monograph concentrates on English society during the reigns of Henry III and Edward I—a period that Lloyd rightly represents as the heyday of English Crusading activity.

The central chapter of the book, and in many ways the most impressive, is a detailed investigation of Lord Edward's Crusade of 1270–72. It is of significance for the general study of English military and social development, for Lord Edward's Crusading force was the first English example that is available for study of an army systematically organized by contract. As Lloyd prudently insists, Lord Edward was not a great innovator; it is the bringing to light of what was becoming established practice that makes the Crusade of 1270–72 so interesting. Another example of an important development that this Crusade brought to maturity is the emergence of Crusader attorneys with general powers; legal historians will welcome Lloyd's clear analysis of their emergence.

For the rest, Lloyd covers four major aspects of the impact of the Crusade on thirteenth-century English society: the promotion of the Crusade by preaching and the ecclesiastical machinery, the nature of the response, the practical preparations for the journey that Crusaders made, and the personal policies of kings Henry and Edward with respect to Crusading. The discussion of Henry is particularly thorough and challenging; it deserves to be read alongside the work of M. T. Clanchy and D. A. Carpenter as a contribution to the contemporary revision of the politics of his reign. Lloyd emphasizes the Plantagenets' awareness of a need to emulate the Crusading exploits associated with forebears such as Robert Curthose and King Richard Coeur de Lion: a combination of dynastic pride, regal dignity, and a growing sense of "national" honor impelled the English kings to vie with the Capetians. The debacle of Saint Louis's Egyptian Crusade acted as an incentive rather than as a discouragement; indeed, Lloyd points to Henry III's opportunism in seeking to turn it to his advantage. Lloyd offers a penetrating analysis of the moral and political complexity of Henry's Crusading policies, and he draws an effective contrast with the relative simplicity of his son's when he was king.

Even in the thirteenth century, England's contribution to the Crusade was limited, but Lloyd succeeds in demonstrating that the Crusade's impact on English society was profound and pervasive. His pioneering work is an indispensable guide to the subject.

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GEORGES DUBY. *The Legend of Bouvines: War, Religion, and Culture in the Middle Ages*. Translated by CATHERINE TIHANYI. Berkeley and Los Angeles: University of California Press. 1990. Pp. 234. \$32.00.

This book provides an English translation of the author's *Le dimanche de Bouvines: 27 juillet 1214*, which was published in 1973 as part of the series entitled "Thirty Days That Created France." The armed confrontation in Flanders on a Sunday in July between the French king, Philip II, and Holy Roman emperor, Otto IV, certainly ranks as one of three epochal European battles of the early thirteenth century (the other two being Muret and Las Navas de Tolosa). But Georges Duby provides more than just another political or military assessment of Bouvines; in a fashion that his readers have come to expect, the author also offers both a slice of the medieval experience and a consideration of the battle's subsequent "survival" as part of the French political "folk consciousness." Thus, his approach is partly anthropological, a consideration of the battle itself and of the "changing set" of its subsequent "mental representations" as elements in a distinct cultural milieu (p. 6).

After a brief introductory segment, Duby groups his eight chapters in three clusters: "The Event," "Commentary," and "The Legend." The initial segment contains two chapters, the first of which introduces the reader to the political preliminaries, the major personalities involved, and the setting (both physical and temporal) of the fight. The next chapter comprises what is arguably the heart of the book, a translation into English of the Old French version (ca. 1274) of an eyewitness Latin account of the battle written by William the Breton, one of Philip's chaplains. The inclusion of the later derivative version perhaps made sense in Duby's French monograph of 1973, but less so in this English translation. Would it not be more fitting here to translate William's original Latin account directly into English?

The second cluster contains four chapters, two of which constitute completely independent essays on truce-peace keeping and war making during the two centuries before 1214. Following this sociology of armed conflict, Duby considers in two further chapters the battle of Bouvines as a ritual duel or judicial ordeal and determines how various medieval narratives reported and assessed the French royal victory. The birth and subsequent life of the "myth" of Bouvines engage the author's attention in the two chapters of the final cluster.

Duby ranges over a variety of topics: the armament and numbers of the combatants at Bouvines, the mutual cohesion (familial, vassalic, territorial, stipendiary) of each of the armies, just war theory, tournaments, mercenaries, war tactics in general, and the "liturgical" aspects of battle. Several of the author's observations deserve special attention. Duby notes that the peace-truce of God movements at first grew rapidly in those areas where royal or princely power

was weak and only later spread to regions where kings and great lords gradually assumed dominant roles in the maintenance of order. The arming of the middle and lower classes to defend the peace always risked a potential subversion of hierarchical social values. The Capetians adroitly avoided this danger by firmly controlling the military capabilities of northern French communes. Tournaments provided a training ground for new styles of lance combat, an outlet for aristocratic aggressive instincts in an increasingly orderly society, activity for aristocratic "marginals" (younger sons and other nobles who had not yet settled down in marriage or inheritance), and an opportunity for distributive exchange (that is, the plunder of arms, horses, and ransoms) in an increasingly mercantile economy.

As for Bouvines itself, the author suggests that the diverse composition of Otto's forces adversely affected their battlefield unity. Victorious Philip's vast spoils in captives were certainly remarkable. Most chronicle accounts depicted the battle itself as a type of tournament, with no attention given to those supernumerary combatants not featured on the "main card." The early accounts partial to the French king were unanimous in portraying the results as a victory of the just over impious opponents whose hubris forced combat on a holy Sunday and whose ranks included many hundreds of accursed and sacrilegious mercenaries. Yet soon the first mythic elements intruded. The actual combat took on epic proportions as a titanic struggle of good versus evil, which hinged on the personal duel between Philip and Otto; new accents were provided by a proud French nationalism and an antipathy toward German "barbarians." Philip himself was portrayed as a priest-king, a new Melchizedek, nay even a new Christ who allegedly wagered his royal crown in a supreme act of self-sacrifice.

Philip and the memory of his triumph were eventually overshadowed by the glorious reign of his sainted grandson, Louis IX. Historical interest in Bouvines only resumed in the seventeenth century, whereas its true renaissance dated from the time of the July Monarchy. Here romantic medievalism, anticlericalism, and idealization of the bourgeoisie's historical role (the northern French communal militias) all had their ways to celebrate the victory. Between France's military humiliations in 1870 and 1940, another old element reappeared in the tale, namely anti-German sentiment. For Duby, the mood of post-1945 Europe has finally consigned Bouvines to popular oblivion.

Three caveats are in order. Catherine Tihanyi's translation is eminently smooth and readable, but I wish that she would not render certain non-French place names by their French forms (Aix-la-Chapelle [p. 27], Basse-Lorraine [p. 28], Trèves [p. 57]). Similarly, such appellations as Luther (for Lothario da Segni [p. 33]) and Benoît (for Benedict of Montecassino [p. 98]) are jarring. Peter the Venerable is

seen in both French (p. 64) and English garb (p. 76), and King Stephen of England might not be clearly visible to the reader as (arguably correct) Etienne of Blois (p. 77). Two outright mistranslations occur when *Apostole* is rendered as "Father the Apostle" (p. 40) instead of Apostolic Father and *la glose de decret* as "the text of Decree" (p. 60) instead of the *Glossa ordinaria* on the *Decretum*. All of the Latin texts presented in English in the appendix of documents have been translated not from their original language but from French translations of the 1973 edition. Surely the University of California Press could have exercised greater editorial supervision over such details in a volume that is otherwise so admirable.

ROBERT C. FIGUEIRA
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EDWARD POWELL. *Kingship, Law, and Society: Criminal Justice in the Reign of Henry V*. New York: Clarendon Press of Oxford University Press. 1989. Pp. xi, 319. \$65.00.

This book by Edward Powell on criminal justice in early fifteenth-century England is yet another indication that the fifteenth century is no longer that ignored, forgotten, last medieval century dismissed long ago by William Stubbs as being barren of any significant legal and constitutional achievement. Since World War II, the fifteenth century has received the scholarly attention it deserves from many of the most gifted medievalists in England and the United States. As Powell has so aptly expressed it, the fifteenth century is no longer "the 'Cinderella' of the centuries of English history" (p. 1). Acknowledging the role of the late K. B. McFarlane as a kind of first cause of the torrent of publications since 1945, Powell notes that McFarlane made slight use of legal records and showed little interest in legal or political ideology. McFarlane was convinced that politics in the late Middle Ages revolved around personal connections, affinity, and patronage, whereas in this book Powell stresses that legal ideology and the judicial system were the *sine qua non* of Henry V's success in strengthening the Lancastrian dynasty and in wielding effective power over a kingdom from which he was so often absent.

Based on an admirable command of the archival records and of legal procedure, Powell shows in much detail how criminal justice operated, especially on the local level in the counties, and in the process he substantiates the interesting conclusion reached some years ago by T. A. Green in his *Verdict According to Conscience* (1985) that late medieval juries rendered much equitable justice, that they rendered merciful verdicts that reflected current, popular, and local attitudes toward crime and punishment. In an exhaustive examination of the superior eyre of 1412 in the midland counties of Leicester, Stafford, Shropshire, Nottingham, and Derby, Powell shows that of

the 3,505 individuals summoned to appear in court, 2,706 never appeared. As for the 799 who did appear, 371 were pardoned, and 291 were fined. Of the 137 defendants who opted for jury trial, only three were convicted. This evidence plus much more leads Powell to the conclusion that Henry V followed a conscious policy of pardoning and levying fines. His pardons were issued to all manner of classes, from magnates and gentry down to humble yeomen, a large majority of whom then served him loyally in his French campaigns. And of course the fines collected increased the financial resources that undergirded his military efforts. In this policy Henry V was only doing what Edward I and Edward III had done.

If there was little criminal justice meted out in the common law courts, it was because of what Powell calls Henry's policy of conciliation and recruitment. In order to bolster the shaky Lancastrian hold over the realm and to produce loyal soldiers, Henry had to appease and compromise. He promoted arbitration as a means of settling criminal and all sorts of other disputes, many among the leading magnates and gentry; local arbitrators knowledgeable about the disputes worked out settlements among the disputants. Powell contends that arbitrated settlements were more equitable and enduring than those settled in court. In any event, by supporting expeditious arbitration for local disputes and by pardons and fines, Henry managed not only to put behind him an army that brought him victories in France but also to direct aggressive energies at home across the Channel.

For these achievements Powell gives Henry V high marks: "No king came closer to embodying the contemporary ideal of the just king than Henry V. His success in the administration of justice must rank with his greatest achievements" (p. 275). But one puts down this learned book with the feeling that Powell is consistently too euphoric over Henry's accomplishments. After all, he does admit that Henry made no striking institutional or legal changes and that his conciliatory policies brought no permanent solution to the problem of public order. In fact, seen through less rosy lenses, early fifteenth-century England shows much disorder, no diminution in the evils of retinue and affinity, and areas throughout the realm where magnates with their affinities were the power brokers. It would appear that Henry V supported conciliation mostly for his military adventures and only secondarily for justice.

One final comment. Powell admires McFarlane's "dissatisfaction with the institutional and administrative bias of English medieval history" (p. 2) à la Stubbs and his emphasis on the need to know how royal authority worked out on the local level of the counties. But despite this viewpoint Powell has written a book that does not stray far from the traditional path of constitutional and legal history. He has indeed much enlightened us on criminal justice in the counties but in so doing has only demonstrated in detail

what Stubbs asserted over a century ago, namely, that efficient royal government depended on how effective and close were the ties between the monarchs and their subjects in the counties.

BRYCE LYON
Brown University

DAVID HERLIHY. *Opera Muliebria: Women and Work in Medieval Europe*. Philadelphia: Temple University Press. 1990. Pp. xiv, 210. \$29.95.

In a book whose modest length belies its ambitious aims, David Herlihy proposes to explore, and explain, the "dramatic" changes in women's "productive enterprise" from the early Middle Ages to their end. Emphasizing women's work beyond the immediate needs of family and household during this long period, he notably lengthens our perspectives on changes that have been a principal focus of scholarship for more than a century: the remarkable expansion and then contraction of women's economic opportunities in late medieval northern cities. Recently illuminated by new lines of inquiry pursued by Martha Howell and others, the changing patterns of women's work in late medieval centuries appear here as the culmination of an evolution beginning many centuries earlier.

Glancing first at working women in antiquity, the author traces the long rise of their "prominent participation" in the most diverse endeavors from brewing to "high administration" and "spiritual counseling" (pp. xi, 118). He ends by summing up the apparently swifter decline of this activity in the late Middle Ages, most strikingly in the cloth production traditionally regarded as women's work *par excellence*. Cloth making provides, indeed, a dominant pattern in the somewhat loosely woven fabric of a text descriptive and anecdotal as much as analytical, drawing strands of evidence from the most diverse sources, often fragmentary and occasionally ambiguous in their pertinence to specific issues.

Giving greater depth to this panoramic tapestry are two case studies, the first pursuing the shadowy history of the *gynaecea*, or women's fabric shops, to their late flowering on the great estates, secular and ecclesiastical, of the early Middle Ages. Whether or not we may see in the *gynaecea*, as argued here, rather than in households large and small, the "central institutions" of women's work for two millennia, these female sweatshops marked an important stage in its development. Far different from this exploitation of servile labor in a rural economy were the diversely productive activities of women in the craft economy of Paris in the late thirteenth century, reflected in the second case study, a computerized analysis of contemporary tax records. Remarkably informative about the numbers of women in various occupations, often of lowly status, this study reveals less about the places, conditions, and terms of their work and still less about

the transformations so visible on this lively metropolitan scene.

What we miss is an interpretive framework that would place the changing gender divisions of labor more firmly in their larger contexts of economic and social change. Such a framework would surely improve the somewhat imperfect fit between the conclusions packed into the book's final pages and the text that precedes them, in which the stitches of guesswork and generalization stand out at times too prominently. Perhaps most seriously misleading are observations on "convents" of women, described in general as "few and poor," a judgment inviting the substantial corrections supplied by recent, more broadly based research on women's communities. Among surprising attributions we find, on a single page (p. 110), the poet, Marie de France, identified with the late twelfth-century Countess Marie of Champagne, and Chrétien de Troyes assigned authorship of the earliest French romances; an answer to the problem of "Trotula" (pp. 104-07) awaits the publication of Monica Green's forthcoming work.

Those who share Herlihy's belief in the importance of medieval women's work will welcome the contributions of this sprightly book, whose most significant insights point toward the study in depth that this subject demands.

MARY MARTIN MCLAUGHLIN
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GAIL MCMURRAY GIBSON. *The Theater of Devotion: East Anglian Drama and Society in the Late Middle Ages*. Chicago: University of Chicago Press. 1989. Pp. xiv, 252. \$34.95.

Two big themes inform this ambitious monograph: one concerned largely with the history of spirituality, the other with literary history. Although the second was the original impetus for the work, the first seems to have captured the author's most vivid interest; it is here that the book's greatest contribution is made.

It is not that the impact on literary history is negligible. Gail McMurray Gibson argues convincingly that the eponymous "N" of the so-called N-Town cycle of mystery plays is most likely Bury St. Edmunds and goes some way toward establishing the monks of that great house as the primary patrons. But, as she acknowledges, her "study of the fifteenth-century habits of mind that shaped a text like the N-Town cycle has become an effort to reconstruct an accurate overview of fifteenth-century English religious culture" (p. 1). The result of this attempt at a survey will need to be taken seriously by, at the least, all students of late medieval England.

The primary approaches are biographical, visual, and literary. The biographical is focused particularly on a long chapter in which Gibson ponders the lives of three East Anglian figures: Anne Harling, a literate and strong-willed member of the Norfolk aristoc-

racy; John Baret, a Bury merchant whose splendid tomb monument adorns St. Mary's church there; and a Suffolk merchant, John Clopton of Long Melford, who endowed his village's magnificent parish church with its amazing, free-standing Lady Chapel. For each one there survives a detailed will as well as a benefaction that can be looked at, and the careful analysis of these benefactions in the light of the wills is one of the book's most convincing parts.

Indeed, the visual aspect is beautifully handled throughout, not just as an illustrative frill but as integral to the argument; late medieval piety was intensely visual. The fifty-two shrewdly chosen illustrations, each printed near the paragraph to which it is relevant, make Gibson's points again and again. Long Melford church is the most heavily and effectively drawn on object and is treated as an extended stage set for the "devotional theater," which is the book's chief concern and controlling metaphor.

On the literary side, the most detailed attention is paid to Margery Kempe in a long chapter, the relevance of which to the main argument is not entirely clear, save that she is said to have "possessed an unswerving sense of devotional theater" (p. 47). Gibson's treatment of Kempe, although greatly interesting, points up the less sure handling of the theological than of the visual aspects of her topic. The broad idea of "Incarnational Aesthetic," with which the book begins, is more clearly displayed in her treatment of the theme of Mary's "clothing of Christ" (pp. 156–64) as depicted in art than in the tricky area of literary parallels and borrowings (for example, "It is often when Margery Kempe sounds most like her inimitable self that she is, in fact, most the pseudo-Bonaventure" [p. 49]). Medieval theology is a technical business, full of pitfalls into which a statement such as "as the monk's *opus dei* performs its sacramental function" (p. 130) drops, although plausible at first reading.

For the most part, however, Gibson's insights are both sure and refreshing, and the dust jacket blurb, which mentions her book in the same breath as Johan Huizinga's, is not altogether an exaggeration. More from Gibson on late medieval spirituality would be welcome.

RICHARD W. PFAFF
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WAYNE SHUMAKER. *Natural Magic and Modern Science: Four Treatises, 1590–1657*. (Medieval and Renaissance Texts and Studies, number 63.) Binghamton, N.Y.: Medieval and Renaissance Texts and Studies. 1989. Pp. xi, 233. \$18.00.

Wayne Shumaker knows what constitutes "authentic science" (p. 3) and examines material by Giordano Bruno, Martin Delrio, Tommaso Campanella, and Gaspar Schott in reference to it. Because the works studied were written between 1590 and 1657, when

the parameters of knowledge of any sort were extraordinarily fluid and ambiguous, Shumaker's approach fails to illuminate either the historical period or the modern reader. He claims as his thesis "that science developed largely from a natural magic stripped of mysticism" but finds the relationship only in a path that leads "away from . . . obscurantism" (p. 38). One has, of course, frequently encountered some version of that thesis before, but seldom if ever has one found it presented with the ideological trappings of "scientific advance" (p. 15) and an ill-concealed contempt for "archaic baggage" (p. 15), which Shumaker thinks did not lead in the correct direction.

"Progress," however, "was slow and intermittent." Shumaker argues that there were forces both contributing to and resisting the emergence of modern science—as if modern science were somehow the inevitable summit of human history—and attempts in a remarkable final chapter to locate the point at which modern science triumphed, by moving "backward from modernity" (p. 169). Sir David Brewster in 1832 exemplifies almost "uncompromising rationality" (p. 170), and the first edition of *Encyclopaedia Britannica* (1768–71) strikes Shumaker as "rather consistently modern" and usually "anti-occultist" (pp. 172–73). Ephraim Chambers sometimes "appears to be credulous" in his earlier eighteenth-century *Cyclopaedia* (p. 175); Johann Hübner was "by no means so far along as Chambers" in his *Lexicon* (1712 and subsequent editions through 1792) and was "perhaps less forward-looking than Schott" (p. 179), Shumaker's favored exemplar of the correct attitude in the seventeenth century. Completely lost from the analysis is any sense of historical contingency, as well as any sense of the drama of historical changes that served to convert one world view into another.

Shumaker has performed his usual service to scholarship nevertheless in calling attention to a number of treatises from the Renaissance and early modern period that are now little known and little studied, giving substantial synopses in English of the following: Bruno's *De magia*, *Theses de magia*, and *De magia mathematica*; Delrio's *Disquisitionum magicarum libri sex*; Campanella's *De sensu rerum et magia*; and Schott's *Magia universalis*. Shumaker also offers a twelve-page annotated bibliography entitled "Modern and Renaissance Writings on Natural Magic," not exhaustive but certainly useful.

BETTY JO TEETER DOBBS
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MODERN EUROPE

ROBERT WUTHNOW. *Communities of Discourse: Ideology and Social Structure in the Reformation, the Enlightenment, and European Socialism*. Cambridge: Harvard University Press. 1989. Pp. viii, 739. \$49.50.

An extraordinary tour de force ranging from Madrid to Moscow and covering five centuries, Robert Wuthnow's work attempts a synthetic explanation of the most significant ideological movements in European history. Although specialists will quibble with details, as in all such wide-ranging works, Wuthnow has achieved a considerable measure of success in finding a common explanation for these diverse movements. Moreover, his explanation opens new fields of debate in the history and sociology of culture.

Wuthnow argues that new ideological movements "articulate" with their material, political, and intellectual setting. That is, the content of such movements responds to current problems, and their spread responds to the material conditions that underwrite the production of cultural products and the political conditions that lead influential groups to promote these movements. Yet, rather than simply being governed by those problems and conditions, new ideological movements have original elements that lead to transformations in the way intellectual, material, and political conditions are perceived. Thus, "articulation" reflects a creative fitting of movements to current conditions that can (and indeed actively seeks to) change those conditions.

Wuthnow is at his best in demonstrating that neither "autonomous" intellectual developments nor the emergence of particular classes explains the spread and success of these movements. Wuthnow instead points to the articulation between these movements and political conditions. He accounts for the spread of the Reformation to some European states but not others by showing that only where princes were strong enough to overcome the link between the Catholic church and the established aristocracy could the Reformation flourish. Hence, there was success in England, Scandinavia, and central European principalities but failure in Spain, France, and Eastern Europe. Similarly, only where state administrative and professional groups had the resources accruing to an enlarged state bureaucracy, plus a sufficiently "heterarchic division of authority" (p. 178) to render state control of elites problematic, did Enlightenment discourse thrive. Its success in France, Great Britain, and Germany contrasted with a weak or abortive intellectual effort in Holland, Spain, and Russia. Finally, only where conservative political parties embraced both the landed and commercial elite, leaving the leftist political parties entirely to the trade union movement, could Marxist socialism thrive; where landed and commercial elites led opposing political parties and competed for working-class allegiance, no strong Marxist parties emerged. Hence, the socialist party in Germany prior to World War I was strong as compared to the weak contemporary socialist parties in France and Britain.

Wuthnow's analysis demonstrates that ideological movements are best viewed as a combination of intellectual innovations and their realization in the production of cultural products. And, because the

latter requires both resources and groups motivated to produce them, the success of new intellectual movements is always mediated by material and political conditions: "the shaping of ideology is thus historically contingent" (p. 558).

New ideologies, however, are not merely functional "adaptations" to social stress. Wuthnow notes that ideologies, through responses to changing conditions, may act on those conditions to increase stress and conflict. Ideological innovation and material preconditions thus act as necessary complements to produce new movements, rather than as competing explanatory hypotheses. Wuthnow's "articulation" thus presents a sociology of culture that respects historical contingency yet offers strong explanatory value.

There are weaknesses. The most problematic case in the Reformation, the conversion and revolt of Bohemia, is not covered. And the discussion of the Enlightenment in France lags behind the recent advances of François Furet, Keith Baker, and Lynn Hunt. Moreover, Wuthnow is poorly served by the weak index and lack of a bibliography (in the hundred pages of source notes, finding a particular work is more a matter of luck than logic).

Nonetheless, this is a major achievement. Few works in the sociology of culture show such a rich grasp of history or have so many implications for historical explanation.

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PATRICIA A. QUINN. *Better than the Sons of Kings: Boys and Monks in the Early Middle Ages*. (Studies in History and Culture, number 2.) New York: Peter Lang, 1989. Pp. xvii, 265. \$59.00.

The title is appropriately chosen, for the treatment of boys who were to become monks was far better than that of princes in the early Middle Ages. The book is well written; the reader is continually rewarded with each informative page.

As introduction, Patricia A. Quinn gives a summary of books and articles relevant to understanding the reception and upbringing of young children in the monastery, but she asserts that no book has been written on the social significance of Benedictine oblation, the offering of a young child to God by the parents.

There were pagan and Jewish precedents for oblation. Jesus spoke kindly of children; Paul encouraged fathers to offer their daughters to God. The early Christian communities in Egypt, Syria, and Palestine followed the words of ancient and Old and New Testament scholars. The first to give a definite organization to child monasticism was the Cappadocian Saint Basil.

Basil distinguished between orphans and children dedicated to God by their parents and accepted only

the children of nobles. The dedicated ones had an induction ceremony that protected the monks in their possession of the children. In the monastery, the youngsters had special food privileges, slept separately, had their own activities, and could take vows only at sixteen or seventeen when they were sufficiently educated.

The Benedictines were influenced by Basil's *Rule*, but they took from Coptic and Pachomian precedents the acceptance of children of all classes and of making one person in the monastery responsible for their education. The full instruction for Benedictine child rearing was formulated after the Synod of Aachen in 816 and 817.

The author's second chapter consists of an explanation of the plan for the monastery at St. Gall that clearly shows the physical separation of monks and children. Similar in shape to the monks' residence but only a quarter of its size, the children's area gave the masters an easy access to the supervision of the boys day and night. The cloister in the middle of the oblates' residence gave an opportunity for the boys, especially in adolescence, to be alone sometimes. Located between the monks infirmary and the cemetery, the area gave the boys some idea of the uncertainties of the outside world, lessened the pain of having been abandoned by the parents, and promoted their love for their superiors. Although the plan of St. Gall's monastery is included in the book, it would have been easier for the reader if the author had indicated which side was "north."

The images of children drawn in the Utrecht psalter, one of the most important texts for an oblate, is explored together with *The School Life of Walafrid of Strabo*. From the latter, one can see the oblates' attachment to their masters and their peers. From the former, the power of children as miracle workers; their role as a separate group from men and women; their portrayal in the presence of women; their value in pushing their elders toward fulfillment of God's plan for all people; their capacity to bring joy to grownups; their opportunity to become good Christians through the ceremony of oblation; the potential of evil natural fathers as opposed to good masters; and the wicked doing of children who imitate their warrior fathers, are revealed.

Benedict's *Rule*, the commentaries on it, and Hrabanus Maurus's *De oblatione puerorum* are subsequently discussed. The children therein were regarded as spiritual offerings to God, and their education was less rigorous than the monastic discipline. Physical punishment was used only when absolutely necessary and even then dispensed according to age. In other words, the severe floggings were not administered to the small child, and it was emphasized that the masters should show a good example and not use harsh words. The duty of every child was to denounce the slightest failure of his peers, and the duty of the masters was to direct their charges to have their eyes forever lowered and to supervise them

even at night. Reduction to uniformity and limitation of words between children and monks were the order of day. On the other hand, the oblates were permitted generally an hour to play, a special opportunity to sleep after the first meal, and more food than the monks. They were also the carriers of candles and witnesses to the burial of dead persons. They were cared for as "important keys to the monastery's very existence" (p. 144).

The question of homosexuality is thoroughly discussed by the author. From the time a child reached the monastery, he was warned against physical desire, as opposed to passionate friendship. But only youngsters capable of understanding what homosexuality meant were punished for it. Even the penitentials specify more severe punishment to an adult than to a child.

Quinn further delineates all of the psychological factors used in the monastery to prevent this grave sin from occurring. The author believes that the program was largely successful and illustrates her point by citing extant communication between monks of spiritual love to one another. Nonetheless, we have certain vestiges, namely from Peter Damiani in the middle of the eleventh century, that show the dominance of this habit in the monasteries. The situation was deteriorating before the Gregorian Reformation when the communities came to accept recruits of older age.

There are just a few mistakes in the typography of the book: for example, on page 90 "children," and on page 126 "precentor." There is also some confusion about the dresses and the title of the children's chief educator. On the whole, however, the book is reliable, thoroughly researched, and well documented. I recommend it to scholars, graduate and undergraduate students, and also the general public. It is a volume of tremendous worth, one that deserves to be read.

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JEAN DELUMEAU. *L'Aveu et le pardon: Les Difficultés de la confession XIII^e-XVIII^e siècle*. Paris: Fayard. 1990. 89 fr.

After a series of massive, virtually encyclopedic tomes on various aspects of religious experience focusing on sin, fear, and the search for spiritual and physical security among Christians primarily in early modern Western Europe, Jean Delumeau offers this slight volume on the confession. The study is brief but no less expert and insightful for its brevity. A series of linked essays rather than a comprehensive history of this Catholic obligation, it mainly concentrates on the central theological and practical question that exercised confessors as they entered the confessional box: should indulgence or severity govern their response to penitents?

This theme allows Delumeau to survey the wide literature on the confessional across several centuries—although not quite all of those enumerated in his subtitle—and his reading of it suggests the book's central thesis. From around the time of the Council of Trent to the mid-seventeenth century the dominant posture of confessors vis-à-vis penitents was one of indulgence. The theological justification for an attritionist path to pardon, suggested by such churchmen as Duns Scotus in the thirteenth century, combined with a Tridentine concern not to harry the faithful with an impossibly difficult standard of repentance, yielded an image of the confessor as fatherly and indulgent, prepared to accept the penitent's expression of guilt without any assurance of his heartfelt renunciation of sin. There were, to be sure, many theologians and churchmen, most notably Charles Borromeo, who vigorously objected to this "laxist" position. But it was not until the mid-seventeenth century that a reaction set in. The Jansenists, of course, led the campaign against the attritionist and laxist positions, but the testimony Delumeau presents underscores the fact that their objections were part of a broad-based movement in the direction of confessional severity. The evidence assembled in this context for once allows us to see the Jansenists not so much as beleaguered sectarians but rather as the vanguard party of a more self-confident and thus more demanding church. Delumeau suggests, however, that in the eighteenth century the dominant clerical view of confession again changed course, indicating a retreat from rigorism and a return to indulgence toward the penitent.

Unfortunately, Delumeau does not offer any explanation for these several shifts in the church's attitude and clergymen's practice. But this is only one of the book's obvious deficiencies. "The difficulties of the confession" are discussed solely from the perspective of the clergy, and although this allows the author to present some fascinating testimony of priests' anxieties and misgivings (summarized in a chapter neatly titled "Spiritual Obstetrics") as they approached the often onerous task of confessing, the experience and attitudes of the laity are virtually ignored. And, although Delumeau is not the sort of historian from whom one would ordinarily expect a discussion of such metahistorical thinkers as Michel Foucault and Norbert Elias, in a book that begins with several mentions of Sigmund Freud and ends with some reflections on the role of the confession in forming the particular self-consciousness of the West, the reader nevertheless might be forgiven for expecting something of this sort. More glaring is his failure to engage the views of other historians who have written on the confession, most notably Thomas Tentler and John Bossy. In short, this is a book suspended in a virtual historiographical vacuum.

But it is also a book rich in erudition, solid reflection, and, most refreshing, in Delumeau's partisan concerns as a committed lay Catholic. His preference

for a faith that offers psychological reassurance and spiritual healing to the penitent is unobtrusively apparent throughout the work, making it that rare sort of book—both scholarly and personal.

ROBERT SCHNEIDER
Catholic University

KEITH MOXEY. *Peasants, Warriors, and Wives: Popular Imagery in the Reformation*. Chicago: University of Chicago Press. 1989. Pp. xiv, 165. \$29.95.

In the same way that literary studies are now broadening to include genres that have not traditionally been considered "literature," analyses by art historians are now investigating genres that were either ignored or relegated to the status of "folk art" in the tradition descending from Erwin Panofsky for which aesthetic considerations were of prime importance. Keith Moxey's study is a welcome addition to this new style of art history, analyzing three types of images found in woodcuts of the sixteenth century. The author clearly states his premises at the outset, noting that he views these woodcuts neither as folk art representing the views of lower-class people nor as propaganda produced for the masses by the upper classes but as a "structure of ideological sign systems" (p. 8) that both reflected and shaped social reality.

Each of the central chapters focuses on a single woodcut produced in Nuremberg during the sixteenth century. Moxey provides introductory chapters both on the organization of production of these types of images and the social status of their producers and on the social and political background of the free imperial city of Nuremberg, a place where life was governed down to the last detail by a small group of patrician families. The first woodcut is Sebald Beham's *Large Peasant Holiday*, produced in 1535, which Moxey compares with Beham's earlier work and contemporary pieces by other artists as well as with pamphlets, sermons, and moral stories about peasants produced in the aftermath of the Peasants' War; he arrives at a quite different evaluation of Beham's attitude toward peasants as expressed in this work than many earlier commentators.

The second woodcut is *A Column of Mercenaries*, produced by Erhard Schön in about 1532, which Moxey relates to other positive and negative images of mercenaries; he explores how these fit with Nuremberg's attempts to maintain a pro-imperial stance despite its becoming Protestant. Again the author sets these images within a larger intellectual context, that of the debate over just war and Martin Luther's theory of the two kingdoms. The third woodcut is much smaller than the first two; it is also by Schön and illustrated a broadsheet written by Albrecht Glockendon entitled *There Is No Greater Treasure Here on Earth than an Obedient Wife Who Covets Honor*, published in 1532. The image shows a man pulling a cart while being driven on by a woman

holding a whip followed by a young man and woman, a woman dressed as a fool, and an old man. Moxey nicely explores the iconographical tradition surrounding each figure, compares this woodcut with other images of insubordinate wives and abusive husbands, and sets the gender politics portrayed within the framework of Lutheran ideas of the proper sexual hierarchy in marriage and the changing economic role of women.

Moxey has provided much for both art historians and historians to mull over, demonstrating the complexity and the ambivalence of sixteenth-century views of peasants, mercenaries, and women; the quality of the reproductions will easily enable other readers to test his hypotheses.

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MANILO BELLOMO. *L'Europa del diritto comune*. Rome: Cigno Galilei Edizioni di Arte e Scienza. 1989. Pp. 241.

Manilo Bellomo has written a history of the common law of Europe that extends back to the codification of Justinian in the sixth century and reaches forward to codifications of the European states of the nineteenth century. The term "common law" sounds strange to most English-speaking historians when applied to continental legal systems. We speak of common law as being the legal system that evolved in England from the eleventh century to the present. It has been happily transplanted to North America. Bellomo would call English, French, German, and other national legal systems *iura propria*, distinguishing these systems from the common law, *ius commune*, that evolved in the schools of Western Europe from the eleventh to the seventeenth centuries. The principal ingredients of the *ius commune* were feudal, Roman, and canon law. In Anglo-American legal history, this common law is most often referred to as learned law to set it apart from law that was not taught in the schools.

The continental *ius commune* has posed two problems for modern scholarship. First, to what extent did the *ius commune* shape each *ius proprium*? Second, did the *ius commune* exercise real authority in the courtrooms of Western Europe? Bellomo's survey provides much information about the first question and, not surprisingly, less about the second, as only detailed studies of local court records can answer it properly. Unfortunately, not enough work has been done in this field of legal history. Few historians have the tools to switch comfortably and skillfully from the *ius commune* to a *ius proprium*. The work is hard but not impossible. In this country, for example, Richard Helmholz and Charles Donahue have written model comparative studies.

Bellomo has written a broad, sweeping history of

the common law, and, when it is translated into English, many teachers of legal history will find it a perfect textbook for their classes. Bellomo's scholarship and judgments are impeccable. Moreover, he has written a textbook with footnotes. Scholars in the field will benefit from the bibliographical survey in his notes. He describes the beginnings of the common law in the schools of the twelfth century, discusses the development of Italian, French, and German law (*iura propria*), emphasizes the importance of the university for the establishment of the common law, and has incorporated sketches of the great jurists who gave the common law its intellectual vigor: Gratian, Accursius, Odofredus, Cino, and Bartolus. He uses a splendid metaphor from the Ptolemaic universe, tinged with medieval papal rhetorical resonances, to characterize the relationship of the *ius commune* and *iura propria* (territorial laws). The *ius commune* is the sun and the *iura propria* are the planets. The sun illuminates and warms the planets and gives them life. The image is perfect—and right. Bellomo concludes his survey with an account of the humanist jurists of the fifteenth, sixteenth, and early seventeenth centuries.

This book is a textbook but a textbook with a vigorously argued thesis. Bellomo describes a "wave of codification" (not the first, as Sten Gagnér and Armin Wolf have demonstrated) that swept over Europe in the eighteenth and nineteenth centuries. He contends that codification is an imperfect instrument with which to grapple with legal change, and Europe's reliance on codification has introduced legal ambiguity, rigidity, and uncertainty in all of the *iura propria*. He believes that a new *ius commune*, analogous to the medieval and early modern common law, would provide a much better vehicle for legal change and development. With 1992 and European unity on the horizon, Bellomo's vision is not nearly as utopian as it would have been five years ago. This is a superb book, with something for almost everyone. Anyone interested in law should read it.

KENNETH PENNINGTON
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THOMAS RIIS. *Should Acquaintance Be Forgotten . . . : Scottish-Danish Relations, c. 1450–1707*. In two volumes. (Odense University Studies in History and Social Sciences, number 114.) Copenhagen: Odense University Press. 1988. Pp. 296; 393.

This richly detailed study of the interactions between Denmark and Scotland in the late medieval and early modern period is a timely publication. It is many years since there was a good book produced on Scottish emigrants to Europe, although the stream was a powerful one to all the Scandinavian countries, as well as to Poland, Holland, and France, before the end of the seventeenth century. Fewer Scots went to Denmark than to Sweden, but many thousands

moved there nevertheless and made a notable contribution to Danish cultural, economic, and military history. The flow in the other direction was, of course, much smaller but included two queens, various courtiers, and a number of Danish students studying at Scottish universities. Whereas most of the Scots who went to Denmark either stayed there or died in military service (unless they went on to serve the king of Sweden), the Danes who came to Scotland generally returned home.

Of the two volumes, the second contains appendices on the population of Danish towns and on shipping movements between Scotland and Baltic ports and includes short biographies of Scots in Denmark (227 pages) and Danes in Scotland (17 pages). The meat of the analysis is in the first volume, where, following an introductory chapter on Scottish-Danish relations, there is one section on "types of Scottish immigration" (merchants, peddlers, craftsmen, military and civil servants, refugees), a second on the Scottish immigrant craftsmen in Elsinore, Malmö, and Copenhagen, and a third on the Danes in Scotland.

One is struck by the early nature of much of the migration, especially that involving merchants and craftsmen. The relative peak of Scottish population at Elsinore (about 9 percent) was reached in 1567, the absolute peak (fifty-one families) in 1597. On the other hand, the military migration, beginning with the attempted recruit of trained Scots in 1519, was notable whenever there was Danish-Swedish hostility but reached its zenith with the intervention of Christian IV in the Thirty Years' War, when Donald Mackay's company alone totaled three thousand men. The English Privy Council was anxious to get rid of unruly highlanders and borderers, and vagabonds, masterless men, and gypsies, but the recruiting officers were also guilty of abducting respectable citizens from their beds and hustling them off to a more or less certain death in mercenary service. Some parishes when instructed to surrender bad characters denied that there were any such within their bounds; others willingly parted with the unemployed and the godless, such as the fifty-year-old man reputed to be a "sorcerer and charmer, refractare to the Kirk" (p. 103).

There is much information here about the sixteenth-century world of learning and of contacts across the North Sea, sometimes between scholars who never met, such as Brahe and George Buchanan, sometimes through Scottish emigrants who worked in Danish university life, such as John MacAlpine who helped to formulate Danish Lutheran dogmatics. Ambassadors had a pivotal role in cultural as well as political contact, none more so in the reign of James VI than Sir Peter Young on the Scottish side and Niels Krag on the Danish, mutual friends of Tycho Brahe. With the marriage of James and Anne, relations between the countries were particularly close,

and a Danish nobleman translated both *Basilicon Doron* and the *Daemonologie* into Latin.

Finally, the economic historian will find matters of interest in the commercial contacts, not least in the place of Scots in Elsinore and the unexpected levels of Scottish trade with Aalborg before the foundation of Gothenberg. This is an excellent book, with something for almost everyone interested in the history of either state.

T. C. SMOUT
University of St. Andrews

CAROLINE CAHM. *Kropotkin and the Rise of Revolutionary Anarchism, 1872-1886*. New York: Cambridge University Press. 1989. Pp. xii, 372. \$49.50.

This is an excellent book that focuses on issues in the international anarchist movement and Peter Kropotkin's thoughtful contributions to their solution. Caroline Cahm's interest lies in fitting Kropotkin into the milieu that he entered with his emigration to Europe in 1876—into the waves and tides of international anarchism—although she is also well aware of earlier influences on his ideas. Cahm's unusual approach is by topic rather than by biographical chronology. She knows her sources thoroughly; she has followed anarchism through its international conferences, national deviations, and individual leaders, using speeches, letters, periodicals, and conference records. The result is that she speaks to her topics with an inclusiveness almost encyclopedic, a technique that increasingly evokes the reader's fascination. This detailed information serves as an excellent addendum to Martin Miller's fine biography.

Cahm divides her material into three sections, each of which concerns one or more of the striking and divisive problems facing the anarchists of Europe in the years between 1870 and 1890, the formative period of anarchist theory. In each instance, she reviews the position of the antistatist anarchist international and its proponents (including individuals and national sections) on her major themes before introducing the thinking of Kropotkin, tracing the origin of his ideas, and exploring his relationships, conflicting and sympathetic, with trends in the movement itself.

Cahm's first topic is the development of a definition for "communist anarchism," a concept that grew out of Bakuninism and to which Kropotkin eventually committed; here Kropotkin developed his theories only slowly, although he eventually became the "leading exponent" of the broad social production and distribution that the term "communist anarchism" implied. The author's second section deals primarily with "propaganda by deed," namely, with those individual *attentats* with which Kropotkin sympathized (for their heroism) but which he seldom actively supported; Kropotkin preferred collective action and demanded serious revolutionary intent rather than

public murder for effect. In this regard, he set himself in opposition to the entire tone of the anarchist congress in London in 1881. In her third and final section, Cahm considers Kropotkin's increasing interest in anarcho-syndicalism, whereby the violent, spontaneous strike may inspire the vitally important "spirit of revolt." In between we watch Kropotkin oppose all organizations that reeked of authority, all parliamentary action that smelled of palliatives, and all trade unions that sought no more than immediate economic concessions. In 1886, Kropotkin moved to England, which he hated, and in spite of his congenital optimism, one can sense his depression about the movement. He continued to write a great deal, but he became a kind of loner.

This book leads us once more to a Kropotkin who was an extraordinary individual. What is striking about Cahm's Kropotkin is his unfailingly moral stance, which Cahm credits to his early experiences in Russia and particularly to his association with the Chaikovskii circle. Kropotkin judged all revolutionary efforts against a strong ethical code that insisted on free-spirited, decentralized but not authoritarian organization, on revolutionary violence but not murder, on "expropriation" but not robbery, with all the contradictions and ambivalences that these distinctions provoked.

His later theory that linked anarchism with science, proposing "mutual aid" instead of competition as the basis for biological progress, was in Cahm's view Kropotkin's greatest intellectual contribution to the anarchist movement.

What has become of Cambridge University Press? This important book would be easier to read after careful editorial checking: there are occasional typos, some minor grammatical flaws, two-page paragraphs, and a failure to identify many individuals by more than last name. In my copy, much of the index is rendered illegible by blurred printing. Cahm's informative and detailed manuscript deserves better than that.

DEBORAH HARDY
University of Wyoming

THOMAS W. HEILKE. *Voegelin on the Idea of Race: An Analysis of Modern European Racism*. Baton Rouge: Louisiana State University Press. 1990. Pp. xiii, 161. \$22.50.

This book offers an exegesis of two early works on racial doctrines by the political philosopher Eric Voegelin. *Rasse und Staat* and *Die Rassenidee in der Geistesgeschichte von Ray bis Carus* were both published in Germany in 1933. These books are of interest as fascinating examples of the German ideology. They contain critiques of biological race doctrines from an idealist perspective. Voegelin accepted the validity, even the value, of what he called the idea of race as a means of constituting and unifying a political com-

munity, but he deplored the modern practice of defining race solely by physical characteristics. He rejected the Darwinian theory of natural selection that he believed to be the basis of biological race doctrines. He denounced the positivist assumptions that race can be defined by the methods of natural science alone or that characteristics of the mind can be derived from characteristics of the body. Instead, he extolled the early nineteenth-century racial doctrine of the German physiologist Karl Gustav Carus, for whom the ideal of the well-born, complete man (Johann Wolfgang von Goethe was his model) established the norm according to which races should be classified and ranked.

These early books by Voegelin are fascinating not for what they tell us about race or race theory but for what they tell us about the genealogy of Voegelin's later political philosophy. The major themes of his magnum opus, the five-volume *Order and History* (1956-87), are clearly rooted in the idealist assumptions of his youth. Unfortunately, Thomas W. Heilke's study, written under a grant from the John M. Olin Foundation, is quite inadequate as a guide. This is an uncritical study written by a Voegelin devotee. The subtitle of the book (probably selected by the publisher) is seriously misleading. Also misleading is Heilke's effort to cast Voegelin's works on race as attacks on National Socialism. Voegelin accepted the Nordic myth as a potentially salutary and efficacious political ideal; he only denied that it was susceptible to scientific explanation as biological race theorists seemed to assume or that it could be defined by physical criteria alone. Voegelin explicitly condemned opponents of the race idea as even more guilty of the same offenses for which biological determinists stood condemned. The real objects of his attack were the philosophical materialism and political egalitarianism that he associated with the growing influence of liberalism and Marxism in the nineteenth century.

The continuities with Voegelin's postwar political conservatism are quite striking. The process of secularization ushered in by the Enlightenment is held responsible for the decline of the political and intellectual culture of the West. If Heilke had approached his topic from a historian's perspective, his book could have served a useful purpose. Voegelin's early works offer marvelous insights into the political culture of Germany's prewar Conservative revolution. They epitomize conservative misgivings about the potentially democratic implications of doctrines that determine racial categories on primarily physical grounds. For Voegelin, ideas of race that fail to promote the development of an intellectual (*geistig*) elite lose their value as political ideals. But Heilke contributes little to a clarification of the social and cultural context in which Voegelin's early works originated. A simple translation of these works would

have been more helpful than Heilke's convoluted and misleading explication.

ROD STACKELBERG
Gonzaga University

DAVID LOADES. *Mary Tudor: A Life*. Cambridge, Mass.: Basil Blackwell. 1989. Pp. xiii, 410. \$29.95.

Every biography of Mary Tudor must accept, modify, or rebut John Foxe's famous verdict on the reign: "We shall never find any reign of any Prince . . . which did ever show in it . . . so many great arguments of God's wrath and displeasure . . . whether we behold the shortness of her time, or the unfortunate event of all her purposes" (*Acts and Monuments*, vol. 8, p. 625). David Loades has cleansed Mary's life of Protestant teleology and the opprobrium of having been sterile. He insists that the cause of the failure "of all her purposes" did not lie in the usually assigned reasons: a virulent Protestant minority, an overly large and usually paralyzed council, the influence of a foreign and not always well-intentioned husband and king, and the presence at court of the Emperor Charles's subtle ambassador, Simon Renard. But he is unwilling to accept the new revisionist argument that if Mary could have had another decade to live, then the tensions and disasters of her reign would have receded or been resolved. Instead, his assessment of the queen is as harsh as any given.

He refuses to offer the fig leaf of inevitability—either God or the forces of history were arrayed against her—to conceal what he believes to be the fundamental explanation of most of the calamities of her reign: Mary's own personality. She was her own greatest obstacle. Mary Tudor was stubborn yet "hysterically indecisive," naive but neurotically suspicious, and pathologically dependent yet dictated by the uncompromising voice of conscience. Although capable of moments of great heroism, she lacked the charisma, style, and timing to impress her subjects with a sense of leadership. She was a theatrical dud and totally devoid of any "objective political vision" (p. 328). She governed by a "series of moral reflexes" in a world in which pragmatism, compromise, and realpolitik were required to survive. A profoundly contradictory and complex figure, Mary was a victim of her past that prepared her for a martyr's crown but presented her instead with the crown of England, for which she was hopelessly unfit.

Willing to acknowledge but never to dwell on the "achievements of the reign," Loades has no hesitation in assigning blame for the Smithfield fires: "The queen's direct responsibility for the burning of heretics is clear, and in some cases applied with a zeal which smacks of vindictiveness" (p. 236). The woman who is portrayed in these pages is well-meaning—at least in her own eyes—but deadly dull, inept, and anachronistic. She is a sovereign for whom everything went wrong that could go wrong. Oddly enough, this

assessment in a modern secular sort of way is not so far removed from Foxe's sixteenth-century estimation. Although not everyone will agree with the interpretation, Loades has written by far the best biography of the queen to date. He has created a deeply moving and penetrating human story in which the lesser characters retain their verity and are gracefully integrated into a drama that is, as the author says, "pure tragedy."

LACEY BALDWIN SMITH
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DAVID CRESSY. *Bonfires and Bells: National Memory and the Protestant Calendar in Elizabethan and Stuart England*. Berkeley and Los Angeles: University of California Press. 1989. Pp. xiv, 271. \$30.00.

David Cressy's important and entertaining book provides a mass of useful information and addresses fundamental issues in recent historical writing on early modern England. On the most basic level, Cressy reviews the calendar customs of early modern England, explaining the timing, religious significance, and ritual celebrations of all of the major festivals of the year. As Cressy emphasizes, the ritual calendar was fundamental to Elizabethans' conception of time, and it is hard to make sense out of documents and works of literature without a firm grasp of the facts that he presents lucidly and concisely. This study provides the best and most pleasurable account of them.

But the book is far more than a text for the calendrically ignorant. Cressy also challenges and transcends the work of earlier writers on the subject who have persistently presented the old calendar as an unchanging expression of a traditional, agrarian society. Indeed, the view that there was virtual stasis in popular culture and social life threatens to become orthodoxy among English historians, despite the fact that it is manifestly false. Cressy proves beyond dispute that there were, in fact, significant alterations in the festive rhythm of early modern England. Old observances were suppressed during the Reformation; new national holidays were added by the crown and church. The changes that resulted in the construction in the seventeenth century of a Protestant, patriotic calendar unique to England is Cressy's central theme.

The most conspicuous changes were the creation of holidays marking the great political events in the history of English Protestantism, notably, the defeat of the Spanish Armada and the failure of the Gunpowder Plot in 1605. Cressy traces in fascinating detail the origins of these anniversaries and describes the meaning of the changing customs for observing them. He shows how government sponsorship could create a new popular custom only when the holiday crystallized the attitudes (not to say the anti-Catholic bigotry) of the common people. The force of public

opinion was particularly evident in the contrasting fates of the birthday celebrations of the Tudor and Stuart monarchs. As Cressy's innovative analysis of the payments that churchwardens made for bonfires and bell-ringing demonstrates, Good Queen Bess merited a larger, noisier, and longer-lasting bash than her successors.

The surprise here is that political events, government action, and popular political attitudes played a large part in shaping the changing conceptions of the meaning of time and the relationship of the present and the past. This argument is Cressy's second major challenge to social and cultural historians, and it is also successful. For some time now, critics have complained that social historians have failed to integrate history into their histories and that they do not appreciate the importance and pervasive influence of politics. Most of us have nodded wearily; some have grumbled about a reactionary turn in historiography. Few have seriously tried to achieve a rapprochement of political and social history. This is precisely what Cressy's book does, and it should serve as an example for others to follow. His bonfires light the way; his bells summon us to action.

MICHAEL MACDONALD
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CHRISTINE MACLEOD. *Inventing the Industrial Revolution: The English Patent System, 1660–1800*. New York: Cambridge University Press. 1988. Pp. xii, 302. \$44.50.

This is the second book on English patents to appear in recent years, and therein lies a story. In 1981 the late Harold Dutton completed his doctoral thesis on English patents. It was published as *The Patent System and Inventive Activity during the Industrial Revolution, 1750–1852* (1984). In 1983 Christine MacLeod finished her thesis on patents from 1660 to 1753. She then apparently took a calculated risk and delayed publication while she revised the text and extended her research to 1800, thereby putting her work into competition with Dutton's book. Was the chronological extension of her text worth the delay?

The answer is an unequivocal yes. To be sure, both books are the results of prodigious research in manuscript and printed primary sources, and both make significant contributions to the field. Dutton spent about half of his book dealing with the campaign for patent reform in the first half of the nineteenth century and other post-1800 developments. He also provided illuminating contrasts between patenting activity in Manchester and Birmingham. MacLeod furnishes a much fuller discussion of late eighteenth-century patenting than does Dutton, and by providing a detailed analysis of London patents, she nicely complements Dutton's provincial case studies. Although MacLeod's book is significantly longer and

arguably better than Dutton's, the two works together provide indispensable guides to English patents from the middle of the eighteenth century to the mid-Victorian period.

MacLeod offers her readers much more than that, of course. Within her compass lie early patenting practices, starting with the Elizabethan patents used to stimulate the introduction of new industries and techniques from abroad. Under the Stuarts, this policy degenerated into a practice of granting patents as royal favors to provide monopolies within preexisting industries. The author proceeds to trace how a recognizably modern system evolved in the eighteenth century primarily as a result of changing legal decisions. It ultimately grew into a Dickensian bureaucratic maze that provided protection for native inventions regardless of whether they worked or not. Among other things, the author also analyzes the socioeconomic status of late Stuart and early Hanoverian patentees, the changing geographic distribution of patenting activity, and the acceleration of patenting under George III.

MacLeod is explicit about her main conclusion: "If this book does nothing else, it should finally undermine all attempts to build tall superstructures on the fragile foundations of the patent statistics" (p. 2). To that end, she provides a series of revised annual figures for patents (p. 150) and discusses their numerous limitations as indicators of "inventiveness," economic growth, or anything else of significance. Her main argument is that too many inventions and improvements (such as chemical processes and changes in industrial organization) were simply not patented or not patentable.

The flowering of scholarly inquiry into English patents has been paralleled by fresh investigations of U.S. patents (by Kenneth Sokoloff and others) and patenting in other Western countries. Without a doubt, MacLeod's book stands in the front rank of these studies in terms of comprehensiveness and insight. But more work remains to be done. As a first step, MacLeod ought to offer her assessment of Richard Sullivan's various articles and comments on English patents (see especially "England's 'Age of Invention': The Acceleration of Patents and Patentable Invention during the Industrial Revolution," *Explorations in Economic History*, 26 [1989]: 424–52). His work diverges from MacLeod's in terms of basic statistics and conclusions regarding the wider significance of patenting trends.

ROBERT GLEN
University of New Haven

JOHN BREWER. *The Sinews of Power: War, Money, and the English State, 1688–1783*. New York: Alfred A. Knopf. 1989. Pp. xxii, 289. \$19.95.

W. E. H. Lecky observed a century ago that in eighteenth-century parliaments the "questions that

excited the most interest were chiefly financial and commercial ones." Topics such as the national debt, sinking funds, encouragement of trade, the excise, land tax, and Continental subsidies, he continued, "produced the most vehement and the most powerful debates." Few historians of the period would deny the broad truth of this, but most have indulged every temptation to marginalize it. Twentieth-century specialists have had their reasons, as the Victorians had theirs, for not taking the business of English central government in the eighteenth century seriously. The importance of John Brewer's new book is that it should compel everyone to take it seriously.

It does this by offering a neatly integrated portrayal of the bureaucratic and financial accomplishment. Historians are familiar with the Tudor and nineteenth-century "revolutions" in modern English government. This book calls attention to the revolution that happened in between, during the decades prior to 1720. The name "fiscal-military state," which Brewer gives to the product of this second revolution, indicates its limitations and suggests what still remained to be done by the third. (He makes clear that the word "military" in England's case involved an unusually large naval component.)

Brewer chiefly builds on the work of others, integrating and supplementing the findings of recent studies with alertness and discrimination (and generous acknowledgement). His own delving yields a richly documented survey of the excise department and some valuable compilations of statistics. But the compelling force behind the book's message comes from Brewer's awareness of contexts. Every European monarchy of the early modern period wanted to build some sort of fiscal-military state, but none obtained one as efficient and effective as England's. Brewer does not assume, as so many have done, that this kind of state building was an automatic consequence of the challenges of war. He notes that, on the contrary, many Continental powers had warped and burdened their state apparatus by adhering to desperate expedients in the course of sixteenth- and seventeenth-century wars from which England had largely remained aloof. The relative lateness of England's development of war-making capacities was beneficial because it allowed Dutch experience to be drawn upon, and it occurred in a strengthening parliamentary context, where consent was measured out with reluctance and circumspection. On this latter point, the insightful chapter "The Paradoxes of Power" should be required reading for all students of governmental development.

This chapter also indicates the best answer to the parliamentary puzzle suggested by Lecky's observation, and this is the book's second major contribution. If, as J. H. Plumb observed in 1956, "local government, Parliament and the Church were the three bases upon which the pyramid of government rested" and if members of the dominant political class spent most of their time and energy trying to control these,

how can M.P.s have taken so seriously the kinds of issues Lecky itemized? The answer is that they were so fearful of the thing they had allowed to be created. They came to realize that they could not have a truly efficient fiscal-military state (for which the necessity, thanks to Louis XIV, could not be denied) if they actually employed the blunt instrument of refusing appropriations. Therefore, to contain the threat this efficient state posed to traditional political and social arrangements and to property, they pressed unremittingly for public exposure of its transactions. If, however, public scrutiny were to be effective rather than merely obstructionist, discussions had to be well informed and continual. In short, the defense of the Protestant polity and its embedded aristocratic influence induced gentlemen to pay careful attention to financial, commercial, naval, military, and administrative topics.

Viewing the book as a whole, I might venture four criticisms. First, more attention should have been given to the interplay of interests that enabled important foundations of the fiscal-military state to be laid down during Charles II's reign. Second, the capacity of the English parliament (and public) to influence the shape of war efforts deserves greater prominence. Although the distractions caused by public ventilation of strategies and war aims were palpably frustrating, they interjected a salutary measure of restraint, without which England's evolving institutions might have been wrecked as those of Continental monarchies had been. Third, notwithstanding the tilt of recent scholarship, there are good reasons to think that the efficiency of the British army remained more uneven than this book seems to indicate.

The fourth criticism concerns part 4, which addresses the impact of the fiscal-military state, its needs, and its wars, on the British people. One problem is that this section lacks the comparative insights used to enrich the other parts—insights that could have revealed what the British people did not have to suffer as well as what they did suffer under this system. More important, the mining and synthesizing of existing studies is of little avail here: part 4 gets channeled into assessing the complaints of interest groups, into macroeconomic issues that Brewer planned to eschew (p. 166), and into a topical survey of the strains imposed by eighteenth-century wars. None of this advances our understanding very far. As for the strains of war, a different path of research is required to assess them, as well as the complaining they evoked, because their incidence, rhythms, and combinations varied in each war, often quite markedly.

But this is only to say that a proper execution of part 4's task is, in the current state of scholarship, impossible. The book is a distinguished work—of importance not just to eighteenth-century specialists but also to students of governmental development generally. Though authoritative and historically sophisticated, it is written in a fluent and nontechnical

manner that should reach a wide audience. It even has a first-rate index.

DANIEL A. BAUGH
Cornell University

ROY PORTER. *Health for Sale: Quackery in England, 1660–1850*. New York: Manchester University Press; distributed by St. Martin's. 1989. Pp. xi, 280. \$35.00.

This book is a model of a genuinely social history of medicine. It explicitly locates quackery outside of the telos of progress and professionalization where it is regarded as the sleazy, contemptible, but sure loser to a triumphantly victorious, scrupulously pure, and scientifically correct regular medical establishment. "Quacks" for Roy Porter are not epistemological or moral reprobates but the "frontiersmen," sometimes wildly successful, of the exuberantly entrepreneurial world of eighteenth-century medicine. Joshua Ward, purveyor of "White Drops," for example, earned the patronage of George II, a navy contract, and the right to drive his coach-and-six through St. James's Park by fixing the king's dislocated thumb. (Physicians had attributed the condition to gout.) Joanna Stephens—Porter shows that the medical marketplace was once open to women—sold her nostrum for breaking up stones in the bladder to Parliament for a phenomenal five thousand pounds.

In this world of suffering, not only from major infectious diseases but also from the thousands of other infirmities of the flesh, regular medicine offered nothing effective. And Porter might have added that there were no systematic double blind tests of drugs until after World War II; even today the effectiveness of various treatments is often poorly evaluated. In short, most eighteenth-century drugs did not work, but no one could know better.

In this atmosphere all healers ultimately sold something else besides a cure. Physicians, whose hostility to the quacks was exacerbated by the fact that they, too, had to play up to patients to keep their custom, offered careful listening and personalized care to those who could afford it. They generally did not specialize. Quacks on the other hand worked a specific market: venereal disease, cataracts, painful menstruation, abortion, colic, worms, ruptures, and scurvy (a catchall skin and gum disorder not yet associated with lack of vitamin C, that is, citrus fruits). While regular physicians stayed put—they got most of their customers from one locale—quacks toured around setting up shows wherever they went for their particular nostrum. Whereas the regulars advertized discreetly in the press and through small handbills, quacks lived by the newspapers that in turn lived to a large extent off them. The Georgian press is full of announcements for pills and appliances to fix just about anything, most prominently perhaps sexual dysfunctions and venereal disease. Quacks, in short, were mobile creatures of commerce and suffered the

opprobrium of all those who hated and feared the freedoms of the marketplace.

Porter rescues the intellectual losers; he takes the views of the more articulate quacks seriously. James Graham's celestial bed, renting at fifty pounds per night and guaranteed to result in conception, might be silly, but his views on the dangers of drink, dissipation, and weak constitution for the health of the people put him in the mainstream of early nineteenth-century proto-eugenic thought.

The relative decline of quackery was not the result of the therapeutic success of its rivals but of their success in bringing the state onto their side. Licensing laws for physicians meant that only regulars could get the new jobs offered by the poor law authorities, factory inspectorate, and the like. Quackery was supplanted at the periphery by self-consciously radical and alternative medical views that defined themselves, politically and therapeutically, against the medicine of established authorities.

The historiographical lesson to be learned from this book is that medical history, if it is to break the teleological stranglehold in which it is still held, must look not just at the winners—physicians and what they did—but at the patients as consumers, at material culture, at medicine as a business. It must become, as this book is, more capacious, more generous, more eclectic.

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E. A. SMITH. *Lord Grey, 1764–1845*. New York: Clarendon Press of Oxford University Press. 1990. Pp. viii, 338. \$72.00.

Less than a quarter of E. A. Smith's new biography of the second Earl Grey is devoted to his years as prime minister after 1830; the rest treats the preceding forty-odd years of Grey's career virtually all in opposition. The balance is quite deliberate: Smith believes that all of Grey's career is of greater significance than is usually recognized.

Smith could have usefully devoted more attention to the years in office, especially in view of the recent spurt of revisionism that tends to see Grey (for quite different reasons) as someone who had to be gotten rid of so that the true Whigs (whoever they might have been) could get on with their business. What is certain, however, is that unless we have a clear understanding of the significance of the Whigs' long years in opposition—from 1807 under Grey's leadership—we shall never properly understand what happened during their years in power after 1830. Since 1939, when Michael Roberts published his book, *The Whig Party, 1807–1812*, historians have been far too preoccupied with divisions in the party. As Smith realizes, the more important question is not why the

Whigs were divided but why they stayed together. Grey was a large part of the reason.

Smith's remark in his introduction that "Grey became a Whig by accident, and a reformer by miscalculation" (p. 3) is memorable. More significant, however, than how Grey arrived at his political principles is the fact that from his maiden speech a month before his twenty-third birthday in 1787 to the day he resigned his premiership in 1834, the principles were those of a Foxite Whig. This meant that the principles were libertarian, forged in the heat of the struggle surrounding the American revolution. By the mid-1780s the revolution was over, and the complex of issues that had surrounded it had at least died down. But in the same year as Grey's maiden speech, 1787, Charles James Fox committed himself to another cause of liberty: religious liberty as manifested in the renewal of the Protestant Dissenters' agitation for the repeal of the Test Acts. Two years later a further cause was added by Fox's support of the French revolution. After 1801, a combination of traditional sympathy with Ireland and the new doctrine of religious liberty (as opposed to toleration) emerged in a solid party commitment to Catholic Emancipation.

On all of these issues, Grey was Fox's loyal disciple. On parliamentary reform, however, Grey led and Fox followed. As Smith shows, Grey was never basically uncomfortable about having made a personal commitment to the principle of reform. Rather, Grey saw his miscalculation (and here I hope I am being faithful to Smith's argument) in actions of his that committed the party too deeply for its comfort or his own, especially his joining the extraparlimentary agitation by founding "The Friends of the People" in 1792 and by his hopeless motion of 1797, with the Whig secession from parliament that followed its defeat. The result was to create expectations that many of his friends would not and that he, therefore, could not fulfill, save by creating a breach in the party and accomplishing nothing. The serious disillusionment of the reforming public when the Whigs refused to agitate the issue during their brief period in power in "The Ministry of All the Talents" in 1806-07 was the price Grey and the party paid. Never given to idle gestures, Grey would henceforth avoid them like the plague.

The Talents, however, were not devoid of achievements. One was their success in carrying the abolition of the slave trade in 1807. Another proposal, although abortive, was even more important in promoting party advantage. This was the ministry's plan to open all ranks of the army and navy to Catholics and Protestant Dissenters alike. Although prompted by Irish Catholic unrest, the real significance of the proposal (Smith does not make the point explicit) was to cement the Whig alliance with a fast-expanding Protestant Dissent. Practical politics long dictated that religious liberty should be extended first to Irish Catholics—a fact the Dissenting leadership appreciated as well as the Whig elite—but neither doubted

that a victory for the Irish would bring in its wake victory for the principle. Even though the issue caused the Whigs' ejection from office in 1807, the support it gained did much to secure their future.

As Fox's heir in the leadership, Grey was surely right to rally his party around a standard that both maintained unity and brought electoral support. Of those who joined the Whig cause, some left, such as the Grenvillite leadership, but more stayed. At the same time, by discreetly yet firmly maintaining his personal commitment to parliamentary reform, Grey also kept that option open for the party he led.

During the long years in opposition, Grey managed to keep a formidable party together. Briefly, in the last half of 1827, when the Marquis of Lansdowne led the great bulk of the Whigs into alliance with George Canning and Viscount Goderich, Grey seemed to have lost his party. But it came back; thereafter, so long as he chose to lead it, Grey knew no rival. No other leader of the Whigs, before or after, ever led them to greater achievement; that fact is worth pondering.

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DAVID VINCENT. *Literacy and Popular Culture: England, 1750-1914*. (Cambridge Studies in Oral and Literate Culture, number 19.) New York: Cambridge University Press. 1989. Pp. 362. \$49.50.

This book represents the third generation of literacy studies. First we had impressionistic accounts, mostly optimistic, about long-term improvements in the ability to read and write. Next came detailed statistical reconstructions of the actual ability of men and women of various sorts to sign their names in specified circumstances. And now we have nuanced studies such as David Vincent's, which combine qualitative and quantitative evidence in order to evaluate both the context and the consequences of changing literacy levels.

This work is an impressive achievement. Though solidly based on the pioneering research of R. S. Schofield and the work of the Registrar General, Vincent's book moves briskly beyond the signatures and marks of brides and bridegrooms to the political, cultural, and economic circumstances of Victorian and Edwardian England. (Notwithstanding the dates in the title, the book deals mainly with the period after 1839.) Readers will find here a wide-ranging discussion, and a wealth of useful references, on domestic relationships, the penny post, industrial change, labor relations, reading habits, and the endurance of popular beliefs. Rather than finding clear patterns and positive directions, the author sees ambiguities, problems, and contradictions.

At the beginning of Victoria's reign, one in three young men was illiterate (that is, could not write his

signature), as were half of the young women. By the eve of World War I, illiteracy was virtually eliminated, the technical ability to read and to write had become universal. Vincent asks the important questions: what difference did it make? What did people do with these new skills (or tools, as he calls them)? What did mass literacy portend?

In 1839, according to Vincent, the gap between literate and illiterates split England in two; but by 1914 the elementary school system, the penny post, and the popular press had brought about cultural homogeneity and national integration. The transition had profound social consequences. Rapid advances in female literacy, especially in the south where the percentage of literate women overtook the percentage of literate men, "caused a widespread subversion of the established hierarchy of the sexes" (p. 26). Similarly, an age gap emerged, with young people more literate than their elders, which challenged the natural order of the family. Vincent does not develop these observations, but they provide fruitful starting points for further research.

Vincent is rightly skeptical of claims that literacy was a guarantor of progress, the engine of modernization, or a hedge against barbarism. In some cases, it seems, literacy refocused dependency, sustained gullibility, and introduced new mechanisms of discipline and control. Surveying the output of the popular press, with its sensational stories, advertisements, horoscopes, and racing tips, one is tempted to agree with the Edwardian commentator who concluded that mass literacy was contributing to "the intellectual debauchery of the working classes" (p. 270).

Vincent, however, is less pessimistic. He inclines to the view that literacy replaced oral forms of learning and expression, generated new levels of self-awareness, and sharpened the gaps between ignorance and knowledge, childhood and adulthood, magic and science. "From valentines to blacklists, from magical spells to political pamphlets, from dialect poetry to arbitration awards," he argues, "literacy permitted a breadth of engagement which could never be matched by the subsequent revolutions in mass communication of the cinema, radio and television" (p. 271). At the same time he considers "how the State attempted to reconstitute the political nation through the promotion and control of mass literacy" (p. 229). This is a sophisticated book that makes a valuable contribution to our understanding of modern Britain.

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CARL CHINN. *They Worked All Their Lives: Women of the Urban Poor in England, 1880-1939*. Manchester: Manchester University Press; distributed by St. Martin's, New York. 1988. Pp. xii, 187. \$39.95.

Carl Chinn's study of the lives and culture of Birmingham women of the poorest class from the late Victorian period to the eve of World War II is an illuminating work. He brings to his task, in addition to a broad knowledge of the published sources, a deep sensitivity based on intimate knowledge of West Sparkbrook, the neighborhood on which he focuses. Obviously every street, corner shop, and back kitchen is familiar to him. At the heart of the book are the oral history reminiscences of eighty-five people, some of whom are his own relatives and family friends.

The rationale for Chinn's choice of topic is his contention that a distinctive culture separates what can loosely be termed the "urban poor" from the more skilled and secure strata of the English working class. Further, he argues that in a context where male supremacy was weakened not only by the vagaries of unskilled employment but also by the ravages of illness, injury, and early death, women attained a greater social weight than in other classes. They held families and neighborhoods together and became the prime interpreters and executors of a moral system suited to the exigencies of extreme poverty.

Chinn creates a marvelously informative counterpoint between the accounts of social investigators, government reformers, and local historians and the differently seen and expressed accounts of his informants. He is at his best in revealing what he calls a "hidden matriarchy" (p. 35) of women who dispensed advice, managed a complex barter system, offered help in time of crisis, and generally acted as sheet anchors of their neighborhoods. He shows how respected elder women, often dignified by the title of "Granny" or "Mother," became the urban equivalents of the village wise women of an earlier age. In an intelligent exploration of the wide and variegated territory between waged labor and unpaid housework, Chinn incontrovertibly establishes the importance of women's economic role. We learn, for instance, of the extensive but little-known activity of women in the related areas of petty moneylending, pawnbroking and trading in secondhand goods. Chinn tells us, with a precision often lacking in such social histories, how decisions were made within families, what the romantic feelings of spouses for each other were, how the obligations of parents and children were defined, and what happened when a parent became ill or left the household. Our understanding of working-class life is extended, or a nuance added, on nearly every page.

Many readers will wish that this slim volume were longer. Several traditional areas of cultural studies escape the author's attention. For example, he does not discuss religion, either in the form of private belief or public observance. And he does not consider political attitudes or behavior, an odd omission when we consider that most of his subjects, male and female, were enfranchised during the period covered by the book.

A theme that echoes again and again in the oral

histories is the notion that things were different after World War I. Chinn is inclined to discount the significance of the changes. Grinding poverty, he notes, persisted. But, on every topic, from housework to sexual mores, Chinn's informants give the impression that the war was a crucial break between the "then" of Victorian and Edwardian days and the "now" of the modern era. It is regrettable that Chinn devotes only two paragraphs of his too brief conclusion to the impact of the war.

Overall, Chinn's work impressively confirms his belief that the physical and emotional toughness of many working-class women, more than the efforts of philanthropists or reformers, created a culture of survival and self-respect in what might have been conditions of destitution and despair. It is doubtful, however, that many of Chinn's readers or his interviewees would follow him so far as to agree that the primacy of family ties "meant that mothers would control their communities" (p. 165). Chinn's own text gives ample evidence that there were powerful and malign forces, such as the constant threat of illness and premature death, an exploitative and gender-based system of waged labor, male drunkenness and brutality, and social isolation, that were beyond the control of the most heroic of women.

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DIANA SOUHAMi. *Gluck, 1895–1978: Her Biography*. London: Pandora. 1988. Pp. 333. \$34.95.

As interpreted by Diana Souhami, Gluck (Hannah Gluckstein) spent a considerable part of her eighty-two years (1895–1978) struggling to achieve an identity distinct from her family and to obtain perfection in her art and intimate relations. Souhami divides Gluck's life into three periods: rebellion, 1895–1936; love, 1936–44; and the facts of life, 1945–78. Born into a close-knit, rich family, Gluck had a materially comfortable childhood, which was disturbed periodically by her mother's nervous breakdowns. When she was fifteen, Gluck went to a girls' school for a few years, and then her father permitted her to attend a neighborhood art school. Shortly thereafter, in 1916, Gluck left home, adopted male dress, called herself Peter, and began painting seriously. In the 1920s she started having successful exhibitions and became financially secure thanks to a series of trust funds established by her father, whose sense of financial responsibility for his daughter prevailed over his great displeasure with her behavior. In the 1930s, with the beginning of her tortured relationship with Nesta Obermer and the advent of World War II, Gluck's life altered considerably. Although she continued to paint, much of her energy was drained by a variety of problems, and the postwar years brought no improvement. Gluck entered a disastrous relation-

ship with Edith Shackleton Heald, and her brother committed their mother to a mental hospital despite Gluck's objections. Commissions came in at a reduced rate, and her trustees forced her to economize. Eventually, in the 1950s, she stopped painting because of dissatisfaction with the quality of British paints and canvases. After spending the next decade leading an ultimately successful campaign for better art materials, she resumed her painting and convinced the directors of the Fine Art Society to offer her a show in 1973. She died five years after that success and used her will to settle old family scores and to request the staging of a memorial exhibition and the commissioning of a thesis on her work to improve artists' materials.

Souhami's very readable narrative is based chiefly on Gluckstein family recollections and papers, including Gluck's diaries and correspondence, recollections of friends, and the unpublished work of others who have researched Gluck. The book's presentation is commendable; appropriately placed throughout the text are almost one hundred illustrations of Gluck's life and work. A list of the paintings illustrated and brief footnotes, bibliography, and index are included.

Although this is a most welcome study that opens the way for others to examine Gluck and her worlds in a variety of ways, it could have benefited from more theoretical and contextual analyses. Perhaps Souhami intentionally tries to underscore that this is Gluck's story by focusing on her correspondence, diaries, and paintings, but it would be helpful to have Gluck discussed with reference to scholarship about such issues as women's culture, sexuality, class, and gender construction, as well as the British art world of the interwar years.

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DAVID A. PRETTY. *The Rural Revolt That Failed: Farm Workers' Trade Unions in Wales, 1889–1950*. Cardiff: University of Wales Press; distributed by Books International, McLean, Va. 1989. Pp. xiii, 291. \$50.00.

This study is traditional labor history in a Marxist vein. David A. Pretty brings to light the unknown story of Welsh farm workers hitherto obscured by the drama of their bosses'—the small Welsh nonconformist farmers'—economic and political battles, from Rebecca to David Lloyd George. Pretty has fashioned a spirited testimonial to the heroic efforts of dozens of labor organizers, whose stories he has recovered from an exhaustive sifting of newspapers, private papers, interviews with contemporaries, and other relevant materials. The analytical implications of the study, however, provide a curious confirmation of the gaps in Marxist methodology.

The author makes clear the objective class conflict between farm workers and their employers over

poverty wages, endless hours, and miserable housing. Exposure of the farm workers' plight began about the time of the new unionism of 1889–91 in the exposés of an impassioned journalist named ap Ffarmwr ("son of a farmer"). Despite his talents, his attempts to launch a union failed. Generally the workers were held down by ignorance and isolation, and especially by the threat of losing their jobs and the cottages that were often tied to them. But they never lacked dedicated leaders, mostly from outside their ranks—schoolteachers, journalists, the odd minister, sometimes other workers, and at opportune moments organizers from stronger unions, such as the railwaymen, whose higher wages beckoned to farm workers. Real transformation came only during World War I. At first the farmers' wartime prosperity and their "tyranny" over the workers via local poor relief, education, and conscription boards only deepened class hostilities. Then the Corn Production Act of 1917 set up an agricultural wages board with many district boards that negotiated decent minimum wages and sanctioned workers' representation. That official sanction for the first time assured workers that organization would pay off. "Apathy" was replaced by pride, and unions blossomed. But when the boom turned to depression in 1920–21, the wages boards were wound up, and intimidation and hopelessness returned. The boards were reinstituted by the Labour government of 1924, but unemployment undercut wage claims. Nevertheless, wages stood 50 percent above the prewar level, despite the unions' collapse. A similar rise and fall came during the postwar Labour government and the 1950s. The author concludes candidly that more than fifty years of attempts to organize the workers had been a failure.

Having established the material basis for class conflict, Pretty's "intentionalist" emphasis is wholly on consciousness. His categories are inspiration, determination, apathy, resentment; hence, his emphasis on the Sisyphean story of union organization. Indeed, Marx assumed that class conflict comprising poverty and injustice would lead workers to revolt. But Marx lacked a sociology or a psychology that connected popular outrage to collective capacity for action. In this case, consciousness was not decisive in promoting collective mobilization; poverty, outrage, and even leadership were always present but never enough. What was decisive was the demonstration that mobilization would achieve tangible results, thanks to the external factor of government legislation, and the unions rose and fell accordingly. Pretty recognizes this factor but does not allow it to shape his inquiry. Although the wage boards were the necessary condition for collective bargaining—and pride—we hear almost nothing of what went on in and around them. He also passes up the opportunity for comparative analysis, for example, why workers organized more in Anglesey, Pembrokeshire, and Caernarfonshire than elsewhere. We would like to know more about the numbers and distribution of workers and farmers.

Because the focus is on the union organizers, we hear almost nothing from rank-and-file workers. One tantalizing suggestion is that the workers' leaders tended to emigrate (p. 161). Could that have had a cumulative effect over decades?

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PIETRO CORSI. *Science and Religion: Baden Powell and the Anglican Debate, 1800–1860*. New York: Cambridge University Press. 1988. Pp. ix, 346. \$54.50.

Baden Powell (1796–1860) is best known for his controversial contribution to *Essays and Reviews* (1860), that mid-nineteenth-century manifesto of liberal Anglicanism. He would have surely been prosecuted for his piece if not for his sudden death. Whereas historians have tended to focus on Baden Powell's last publications, if they mention him at all, Pietro Corsi, in his first-rate intellectual biography, attempts to reconstruct the complete story of Baden Powell's intellectual odyssey and in the process provides us with a finely textured picture of the changing British intellectual scene from 1820 to 1860. Corsi argues convincingly that Baden Powell, though not an original thinker, was able to respond imaginatively to change by using the ideas of liberal and radical writers to present an Anglican apologetical system fully consonant with a contemporary, scientific world view.

Corsi sees Baden Powell's intellectual evolution in three stages: the early conservative Anglican phase, influenced by his ties with the "Hackney Phalanx"; that phase led to (and overlapped with) the more liberal Oriel Noetic stage; this continued in the liberal direction when Baden Powell became involved with leading London intellectuals. Corsi points out that the notoriety that Baden Powell earned has obscured his early connection with the Hackney circle. Baden Powell did not emancipate himself from their ultra-conservative theological and political standpoint until the mid-1830s. In the late 1820s, he began to develop, Corsi argues, a close friendship with some of the Oriel Noetic leaders, such as Richard Whately, because he perceived that they had elaborated what the Hackney men lacked, namely, a successful philosophical approach to theological and cultural problems that incorporated modern thought yet counteracted the influence of Unitarians and other intellectually sophisticated Dissenters. The early Baden Powell becomes, in Corsi's hands, a symbol of the ties between the Hackney group and the Oriel Noetics, a reminder that we cannot simplistically divide Anglicans of the pre-Tractarian period into conservatives and liberals.

After examining the bonds between the Noetic circle and other sectors of the High Church, Corsi explicates the "dynamics of the widening gap between

the two" as a prelude to discussing Baden Powell's political and intellectual transformations during the decade 1926–36 (p. 61). Both the Hackney Phalanx and the Tractarians at Oriel College, Oxford, including John Henry Newman, Richard Hurrell Froude, and John Keble, resisted the reform spirit behind the Test and Corporation Acts (1828), the Catholic Bill of 1829, the national political debate on the Reform Bill, the reform of the university statutes, and the debate on admission of Dissenters to the ancient universities. But Noetics such as Whately, Joseph Blanco White, and Renn Dickson Hampden, also at Oriel in the late 1820s and early 1830s, desired to accept moderate reform in order to control it. They came to be seen as opportunists in alliance with those attempting to force changes upon the university and the church. Baden Powell sided with the Noetics.

Corsi thickens the plot by noting the seeds of future discord between Baden Powell and the Noetics in their views on science. Where Baden Powell saw that science was acquiring unprecedented cultural authority, and therefore emphasized the need to give science a larger role in the university curriculum to help the clergy meet contemporary challenges to the faith, his Noetic friends remained uninterested in placing science at the center of their Anglican apologetics. Accepting the scientific principle of natural law as the basis for the natural theology at the heart of his apologetic system in 1838, Baden Powell alienated Whately by playing down rational evidence for miracles and later adopting an open-minded attitude toward evolutionary naturalism. It is no coincidence that in the late 1840s Baden Powell began his association with leading representatives of the London liberal and radical intelligentsia such as Francis Newman, William Carpenter, Henry Buckle, Robert Chambers, and Henry Lewes. Stimulated by their ideas, Baden Powell incorporated the internal evidences of religious feelings into his increasingly pluralistic apologetic, moving even further away from Whately's rationalistic "evidential school" (p. 220).

The strengths of Corsi's book far outweigh the weaknesses. Specialists will appreciate the extensive bibliography and the detailed, careful analysis of early nineteenth-century British thought. And, although the thematic structure of the book leads Corsi to some disorienting chronological backtracking, readers should be impressed by the way Corsi weaves his discussion of the broad political and social issues of the day into his examination of the reorientation of philosophical and theological priorities. In his conclusion, Corsi argues eloquently for increased attention by historians to both pre-Tractarian Oxford and the British intellectual life of the 1840s and 1850s. His work certainly provides a good departure point for further investigation into these neglected aspects of British intellectual life.

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JOHN MCCARTHY. *A Last Call of Empire: Australian Aircrew, Britain, and the Empire Air Training Scheme*. Canberra: Australian War Memorial. 1988. Pp. xii, 172. Cloth \$29.95, paper \$19.95.

This book is one of those small and unpretentious works that deal with a minor aspect of great events and throw unexpected light on broader trends and issues. John McCarthy has written a thoroughly professional and, within its terms of reference, satisfying account of the Empire Air Training Scheme (EATS) of World War II. Under it, some tens of thousands of young Australians (and others) joined the Royal Air Force (RAF) and its campaigns, almost all of them in the European theater. McCarthy argues that without them the RAF would not have been able to sustain the scale of its offensive against Germany (although he also points out that by late 1944 the RAF was grossly overmanned in aircrew, even taking account of the expected wastage in final offensives against Japan [pp. 223–25]). He has a wealth of detail on the recruitment, training, and operations of these men, although he also and quite properly points out that "much that mattered was never recorded. Much that was recorded did not matter," and the memories of veterans are notoriously fallible (p. x). But what also emerges are long-term trends of Australia's growing political and strategic emancipation from Britain. One hour after Neville Chamberlain announced that Britain was at war, Prime Minister Robert Menzies told Australians that their nation was, in consequence, also at war with Germany. In doing so, Menzies consulted neither Parliament nor cabinet, but no one offered criticism or even debate (p. 1). As Richard Casey wrote to Menzies in November 1939, "I believe the greatest menace to Australia is the possibility of Britain being beaten in Europe." And the Australian Labor party did not dispute the proposition that in "assisting in the defence of Europe we were defending Australia" (p. 9). By 1945, however, the imperial connection had been found wanting. Australia's political expectations, including the expectations held of the EATS for greater Australian political influence, were not fulfilled (p. 128).

Behind that assessment lies a dilemma that McCarthy does not seem fully to recognize. The Australian concern with Britain and Europe had two facets: the prospective change in the global balance if Germany won the war and Australia's more specific interest in British protection of Australian territory against Japan. That dilemma produced an inevitable divergence in strategic outlook between London and Canberra. For the Australians, from 1939 to 1944, the protection of Australia was a primary war aim. For Britain and the United States, it was secondary. The defeat of Germany and Japan would, ipso facto, make Australia (and everyone else) secure. But that overriding strategic aim would not be advanced, might even be retarded, by an effort to defend Australia. In that fundamental sense the EATS, whatever its atten-

dant political irritations for Australian ministers or commanders, probably made a more direct contribution to the ultimate strategic objectives of Australia and its allies than many other aspects of the Australian war effort.

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ALISTAIR HORNE. *Harold Macmillan*. Volume 1, 1894–1956; volume 2, 1957–1986. New York: Viking. 1988, 1989. Pp. xix, 537; xvii, 741. \$24.95; \$27.50.

With touches of grandeur in style, which his subject would have enjoyed, Alistair Horne has written the official life of one of Britain's most stylish prime ministers—one who was more stylish than substantial. The man who convinced the British in 1959 that they "never had it so good" already looked ineffectual four years later when he left office; since then Harold Macmillan's accomplishment has looked ever shallower. This biography tends reluctantly to confirm that impression.

The story is still absorbing. Horne was commissioned during Macmillan's lifetime to write the biography and availed himself of frequent opportunities to interview his subject over the last seven years of his life. The resulting portrait is memorable. Affluent publisher's son subject to bouts of depression, officer in the Grenadier Guards thrice wounded in the Great War, husband of a duke's daughter, Macmillan led a gilded though not charmed life until 1929 when he was doubly stricken. He was ejected from Parliament by the industrial constituency he loved and deserted by his wife, who remained thereafter faithful to one of his colleagues in the House of Commons. Macmillan was persuaded to keep up the forms of marriage by his continuing, dogged devotion to his wife and by the ambition of his American mother for his political future. The suppressed tragedy steeled but also darkened the rest of his days, at least until the long summer evening of his life after retirement.

Two concerns dominated Macmillan's interest in public life from his army command in Flanders through his summitry forty-five years later with Nikita Khrushchev and John Kennedy. Internationally, he wanted to uphold Britain's interests as a determining power. Domestically, he wanted to spread widely the benefits of the capitalist economy on which Britain's international position was based and from which he derived his own wealth. His successes came on the international side, whether reconciling the French and the Americans while wartime British Resident in North Africa or repairing relations with the United States after the debacle of Suez. Subsequently, however, he failed to reconcile his growing appreciation of Britain's need for membership in the European Community with the special relationships he cultivated with presidents Eisenhower and Kennedy. When planning to visit Moscow at the end of the

1950s in the mounting crisis over Berlin, he kept Eisenhower in touch with his intentions but, in a revealing instance of tactlessness, did not mention them to Konrad Adenauer until the last moment.

His more signal and enduring failure was economic. His patrician sympathy for the working class, which he acquired censoring the letters sent home by the men in his battalion, turned after the war into dismay at the unemployment in his coal-exporting and shipbuilding constituency of Stockton. His response was shaped by the writing of John Maynard Keynes, with whose work Macmillan became familiar as his publisher. This amalgam of social response and economic thought made Macmillan a political centrist, on the margins of the Conservative party in the 1920s and 1930s, often more in sympathy with the Labour party, which, however, he felt it hypocritical as a very rich man to join. Advancing age and his rise to power after World War II did little to moderate his belief in expansionary government budgeting to avoid unemployment and in the worth of centralized economic planning. Far from being warned off by the example of the Soviet Union, he was impressed in the 1930s and again at the time of Sputnik with the USSR's productive growth and technological achievement, which threatened to discredit Western capitalism. Always more apprehensive of unemployment than of inflation, Macmillan refused to heed the warning of the Conservative Research Department in 1961 that the "neo-liberal" tools for governmental management of the economy, including the bank rate and credit squeezes, were worn out. The sequel was Britain's entry into an era of stagflation for which his prescriptions offered no cure.

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FERNAND BRAUDEL. *The Identity of France*. Volume 1, *History and Environment*. Translated by SIAN REYNOLDS. New York: Harper and Row. 1988. Pp. 432. \$25.00.

Fernand Braudel, in his projected multivolume work, undertook to explain "how the long history of France has been constructed in its depths" (p. 25). He chose four vantage points: history and geography, demography and economics, politics, culture, and society, and international relations. The first of these was the vantage point for volume 1, now translated into English. A volume written from the second vantage point and titled *Les Hommes et les choses* was published in France in 1986. The other volumes were left unfinished when Braudel died in 1985.

In part 1 of the volume under review, Braudel describes and discusses France's diversity in geography, climate, language, patterns of settlement, and family structures. Until the development of mechanical transport, this diversity was nurtured and sustained by distance measured by speed of travel, and

France remained "a patch work." "By rights," it has been said, "France should not exist." It had to be invented. In part 2, Braudel shifts to a search for sources of that invention. Seeking them particularly in patterns of human settlement, he takes up, in turn, villages, bourgs, and towns and the similarities and continuities found in them. Finally, in part 3, he debates the classical French geographers on the question, "Was France Invented by Its Geography?" His answer is no. He concludes that "in the process of unifying France then, all the combined forces of history were at work: those of society, of the economy, of the state, of culture" (p. 375).

No review of a few hundred words can adequately convey a sense of the long sweep of this book from prehistory to the 1980s, and if I were to start citing brilliant insights that illuminate shadowed corners of France's past, I would soon run out of space. A review also cannot do justice to Braudel's immense erudition, his ability to call up pertinent details from hundreds of books and articles, or his familiarity with the appearance of seemingly all of France's varied surface. Detachment, Braudel avows, is a cardinal virtue for a historian, but he professes no detachment in writing this book. "The historian can be on an equal footing (*de plein pied*) only with the history of his own country. . . . Never can he enjoy the same advantages, however great his learning, when he pitches camp elsewhere" (p. 15). Although this is a denial of a principal argument of the Pinkney "thesis," I must agree that no foreigner could hope to command the "twists and turns . . . complexities . . . originalities, and . . . weaknesses" (p. 15) of French history and French geography as does Braudel.

Despite my admiration, I have some reservations about the book. Its final conclusion on the unification of France, quoted above, is banal, even trivial, in comparison with the majestic goal that Braudel set for himself at the beginning of the book. The author's intense concentration on the minutiae of geography can be overwhelming and counterproductive for all but the most committed of readers. Twenty-two pages devoted to a detailed, narrative military history of the siege of Toulon in 1707, an episode that had no lasting influence on the long course of France's history, and several other shorter but similar diversions raise some puzzlement over the place of the *longue durée* in Braudel's view of history. What details of the frothy surface of political history might volume 3 of the work have brought us?

But I do not wish to demean Braudel's professional eminence, and I close by noting an unconventional testament to it. On page 120 of this book, Braudel quotes Jules Michelet. His endnote, identifying its source, reads "Jules Michelet, reference mislaid." What historian of our century could get away with that save Fernand Braudel?

DAVID H. PINKNEY
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JANINE GARRISSON. *Les Protestants au XV^e siècle*. (Nouvelles études historiques.) Paris: Fayard. 1988. Pp. 413. 130 fr.

The passions of the sixteenth century are still very much alive in Janine Garrisson's history of early Protestantism in France. The author's intense convictions override her impressive scholarship and her modern metaphors to send the reader back into the thick of the Reformation. "An ever-growing public, informed of the message launched to Christians by the German monk, understands," she writes in the present tense, "that the teaching and behavior of the traditional church is light-years away from the . . . Evangel" (p. 153). Garrisson does treat advanced Catholics much better than the priests of the "religion of panic" (p. 61). She fully recognizes that the Jesuits, like the Calvinists, set out to create more disciplined and introspective Christians. But her praise and her regrets go to the Protestants. As almsgivers, they were able to distinguish the good poor from the bad. As politicians, had they been successful, they could have given France the kind of decentralized system that the Dutch and the Americans, both under Calvinist influence, later enjoyed. One might object that, during the first half of the sixteenth century, France was decentralized and well governed, by the standards of the day. Both before and after the Reformation, moreover, the pressure to centralize came, in large part, in response to the dangers and opportunities presented by religious dissent.

Garrisson wrestles better with the hard problem of disengaging the particular conditions of French Protestantism from the larger patterns of Calvinist Europe. She stresses the pressure to conform exercised by a lay leadership that wanted a tight ship, run from the top down, to impress the crown with the movement's strength, stave off extinction by the Catholic militants, and sort out suitable pastors from among all those who wanted an audience for new religious views. She nicely illustrates the regional differences in the implantation of the reform and offers an instructive set of statistics on who the Protestants were, in social terms, and how, according to their consistories, they misbehaved.

One of the most appealing features in this book, addressed to a broad public, is Garrisson's attempt to portray the new Protestant man, woman, and child. If the men seem rather straightjacketed, as hard workers, authoritarian fathers, and strict masters, then the women emerge with more verve and variety. They are capable of independent thought, whether literate or not, and ready to run risks in search of effective participation in worship and the right to speak—aspirations that, Garrisson regrets, would be far from fully realized in the reformed church.

A. N. GALPERN
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MARC VIGIE and MURIEL VIGIE. *L'Herbe à Nicot: Amateurs de tabac, fermiers généraux et contrebandiers sous l'Ancien Régime*. Paris: Fayard. 1989. 180 fr.

Starting as a botanical and medicinal curiosity in the sixteenth century, tobacco by the time of the French revolution had become an item of general consumption and a significant contributor to French state revenues. In the present century, a number of writers (myself included) have approached the history of this product and its monopolizers from a variety of perspectives: legal, administrative, colonial, commercial. Marc Vigie and Muriel Vigie have very ambitiously attempted to integrate these various approaches into a comprehensive survey of the experience of the French with tobacco in the seventeenth and eighteenth centuries. Their ample bibliography indicates considerable breadth of research extending over several years, although it is much stronger on items published in France than on scholarship published abroad. To make their work more accessible to a wider audience, they have dispensed with notes, an omission bound to disappoint serious investigators following after them.

If disappointing other researchers, the authors have still succeeded in their primary goal of bringing the story of tobacco in France to a wider readership. Their presentation is colorful and vigorous, their focus popular, that is, they are ultimately concerned not so much with the political, diplomatic, commercial, or fiscal history of this branch of the revenue as with the monopoly as experienced by the people of France. Their most original and effective writing is that in chapters 11 and 12 on contraband and its repression. Although this concentration is both interesting and timely, it sometimes can lead to what some less modish readers might consider a distortion of focus. For example, we find twenty-four pages on the brief career of the *contrebandier* Louis Mandrin, about double that devoted to John Law.

The authors have made very thorough use of French printed sources and of some Parisian archival holdings. Their relative neglect of foreign and French colonial materials reflects the internal French focus of their approach. In their presentation, they have deliberately striven to integrate into their text as much as possible of the comments of contemporaries (usually outside the monopoly), whether surviving in private correspondence, literary works, legal opinions and judgments, or administrative proposals. Their presentation of such material is quite effective. By contrast, they have not always been as effective or reassuring with quantitative data. Their omission of notes deprives them of the opportunity of explaining why some of their tables contain figures different from those appearing elsewhere. In one serious case (p. 337), they appear to have converted outside data on consumption per household into consumption per inhabitant, thus roughly tripling the relative volume of tobacco consumption in Paris. Such slips distress

some professional historians but will very likely not appear too important to the wider audience for whom the work is intended.

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BRIGITTE BEDOS-REZAK. *Anne de Montmorency: Seigneur de la Renaissance*. Foreword by ALAIN ERLANDE-BRANDENBURG. (La France au fin des siècles.) Paris: Publisud. 1990. Pp. 415.

This study, designed for the nonscholarly but nonetheless historically knowledgeable French reading public, is an excellent introduction to the complications of the sixteenth century for the historian of other periods of French life. The fact that the career of Anne de Montmorency spanned the lifetimes of several kings gives this study a comparative aspect not always found in the studies of single reigns or in the biographies of Francis I or Henry II. One comes to appreciate, if not always admire, the chameleon-like qualities of Montmorency, constable of France under very different rulers. One sees how often his own and the kings' interests were subordinated to Montmorency's increasing rivalry with and hatred of the Guise and how that rivalry was at times manipulated by Diane de Poitiers and Catherine de Medici for their own advantage.

Brigitte Bedos-Rezak has written an even-handed biography of the major knightly figure in these years, showing Montmorency as the often intransigent, cruel, self-righteous, sometimes even stupid, medieval knight-become-first minister of Francis I and his successors, who was torn by conflicting loyalties between Catholic God, emperor, and French kings. While not denying that picture, or excusing the outrageous acts of Montmorency's administration, the author sees those negative aspects of the constable's character as means toward ends that were often admirable. She paints a picture of Montmorency not only as a great administrator, although a mediocre general, but also as a loyal subject, a dedicated family man coping with the consequences of Protestantism within his own extended family, and especially as a partisan for peace, and less explicitly, for the abandonment of the folly of France's continued Italian adventures.

The wealth of this man was astounding. He owned lands and chateaux throughout France and was the governor of the province of Languedoc, which brought in a huge income except during his years of disgrace. Without being bankrupted he could frequently entertain the royal household or visiting foreign plenipotentiaries at his favorite chateau of Ecouen or Chantilly or at one of his four Parisian residences. He received gifts currying his favor and influence with the kings from powerful lords, bishops, kings, emperors, even from Suleiman the Mag-

nificent, but his enormous resources could also pay to bring crate after crate of ancient and Renaissance sculpture back from Italy. Many of the still-famous treasures of the Renaissance found their way into his collections; he owned, for instance, the sculptural figures by Michaelangelo that had been commissioned for the tomb of Julius II. He ordered tapestries by the dozens and collected medals, marbles, alabaster, enamel, and armaments; he also had a most astounding collection of maps. His power of patronage was important. The artisans most successful in working on any of his chateaux were often promoted to work on royal construction at Fontainebleau and elsewhere. His favorite secretaries and officials were placed by Montmorency in high administrative positions for the crown where they constituted a virtual espionage system for the constable. His intervention could save from condemnation and death a reputed Protestant artisan whose work he admired, but he could also cruelly massacre several hundred citizens of Bordeaux in response to a revolt against the king.

Bedos-Rezak has brought together a wide variety of material to present this picture. She uses not only the letters, speeches, and legal documents of contemporaries and of Montmorency himself but also such varied sources as inventories of the contents of Montmorency's Paris residences and dedications of translations commissioned by him. The author's use of evidence is at times masterly; for instance, she traces the evolution of heraldic devices as a means of establishing that it was not only in his periods of disgrace that Montmorency was concerned with the building of his chateaux or with acquiring objects for his collections. But one wishes that paragraphs were more in evidence, that long quotations had been put into blocks, and that the illustrations were referred to more clearly in the text. Maps need more detail, and plans of buildings would have been helpful. There is an error in the caption of plate 29, illustrating the *gisant* of Montmorency and his wife, Matilde de Savoie—not Louise de Savoie. Despite such minor complaints, this is a worthwhile book based on a fine knowledge of the topic.

CONSTANCE H. BERMAN
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ALAN CHARLES KORS. *Atheism in France, 1650–1729*. Volume I, *The Orthodox Sources of Disbelief*. Princeton: Princeton University Press. 1990. Pp. xvi, 392. \$35.00.

In his classic analysis of mental structures in the sixteenth century published some fifty years ago, Lucien Febvre attempted to explain why atheism was unthinkable in the time of François Rabelais. In this first of two volumes on the progress of disbelief in the following centuries, Alan Charles Kors attempts to explain how atheism became thinkable by the time of Jean Meslier. Unlike previous historians, who have

repeatedly emphasized the role of seventeenth-century freethinkers in the ancestry of eighteenth-century philosophes, Kors argues that Meslier and other atheists borrowed many of their unorthodox arguments from the works of orthodox theologians. Like R. R. Palmer (*Catholics and Unbelievers in Eighteenth-Century France* [1947]), Bernard Plongeron (*Théologie et politique au siècle des lumières, 1770–1820* [1973]), and Dale Van Kley (*The Damiens Affair and the Unraveling of the Ancien Regime, 1750–1770* [1984]), Kors has explored the voluminous literature of sermons, catechisms, manuals, treatises, and polemics neglected in conventional discussions of the French Enlightenment. He does not deconstruct these texts in novel ways, but he does reconstruct the intellectual context in which they were written and digested. He identifies their sources, scrutinizes their logic, and situates them in debates within learned culture during the reign of Louis XIV. Thoroughly researched, carefully organized, traditional in its methods but not in its conclusions, this book does review some familiar material, but it also rewrites part of the script of Paul Hazard's crisis of the European consciousness.

Because seventeenth-century preachers and teachers assumed that no reasonable person could or would question the existence of God, confirmed by the universal consent of all peoples, they used the word "atheism" loosely. They used it for polemical purposes to denounce errors and discredit opponents of all stripes and for moralistic purposes to condemn unrepentant sinners who showed no respect for divine commandments and no fear of divine retribution. The conduct of so-called atheists, who acted like they did not believe in God, reflected willful corruption and not reasoned conviction. It was the orthodox theologians, according to Kors, who elaborated and popularized arguments against the existence of God. Trained in scholastic techniques of disputation, they formulated and published the arguments, of course, for the purpose of refuting them and reaffirming the conventional catalog of divine attributes. Instead of denigrating reason and embracing fideism, the theologians believed that they could demonstrate the existence of God by voicing and silencing "atheistic" objections.

When confronted by apparent evidence of genuine, not heuristic, disbelief in other times and places, the orthodox did their best to minimize the damage. Kors's chapters on seventeenth-century readings of classical texts and travel literature underscore the temerity of Pierre Bayle, who not only denied the reality of universal consent but also insisted that universal consent, in any event, could not prove the existence of God. The more informative chapters on Cartesianism and Aristotelianism analyze the intellectual and institutional issues involved in the "fratricidal" disputes over a priori and a posteriori arguments against atheism, disputes played out not only in weighty theological tomes but also in the more readable pages of journals. Since the orthodox, in the last

analysis, did such a good job of making unorthodox ideas available to the educated public, Kors asks, who needed freethinkers? It remains for him to answer his own question by demonstrating the indebtedness of the self-styled atheists of the Enlightenment to their unlikely seventeenth-century godfathers.

JEFFREY MERRICK
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NANCY NICHOLS BARKER. *Brother to the Sun King: Philippe, Duke of Orléans*. Baltimore: Johns Hopkins University Press. 1989. Pp. xvi, 317. \$24.95.

Nancy Nichols Barker has written about a prince who left neither memoirs nor many personal letters and who not only had no power over day-to-day events but also was an object of ridicule, even contempt, in the pages of memoir writers of his era. Philippe, second son of Louis XIII and Anne of Austria, had to live his entire life under the shadow of his brother, Louis XIV, a shadow that was made darker by Louis's experience with revolt as a young man, his knowledge of his father's problems with a younger brother, and the history of former kings. Preparing a biography of this prince might seem to be a formidable problem, but Barker has developed a life story of Philippe by extensive research in the libraries and archives of Paris, including the archives of the House of Orleans, and careful analyses of modern psychological theories of personality. The result is a book that presents believable psychological reconstructions of the prince's character and feelings and a well-organized picture of his life in the court and field. It is an excellent book that deserves to be read by students of the period as well as by any reader interested in history or biography.

Barker opens with an extensive picture of the royal family from Philippe's childhood to marriage; she follows the neglect and rejection that were his lot, while his brother was trained to become king and assumed the crown. In the development of this picture, Barker ably analyzes the problem of Philippe's homosexual patterns and his social development as well as the origins of his lifelong interests in female activities, clothes, jewels, architecture, and the arts.

His two marriages provide Barker with an opportunity to draw a fascinating picture of the female psychology of these two young women. Barker is obviously able to understand and to explain female behavior cogently. We see that the humiliations and disorder that Henriette piled on her husband only exacerbated an already damaged ego. On the other hand, Liselotte emerges in this study as giving Philippe the only happy period of his life, which happened to coincide with his unexpected victory at Cassel. For a short time Philippe found the self-confidence that eluded him. It is hard to write about his later days when the place left to him at court really

made him into an unhappy buffoon. At the end of his life, however, his German wife again created a situation somewhat more bearable for the unfortunate man.

In view of the subsequent history of the House of Orleans, the chapter dealing with the financial structure of the family is both interesting and valuable. To write it Barker moved from psychology to economics. Based largely on both the Orleans archives and the public ones in Paris, this chapter deals with the rescue and the augmentation of the Orleans fortune that, in the eighteenth century, became the largest personal holding in France. It is a less interesting chapter to read than the others, but it will be welcomed by future students of the House of Orleans.

The study is equipped with an extensive bibliography and index. The Johns Hopkins University Press provided both fine printing and a beautiful hard cover for this provocative study.

JOHN B. WOLF,
EMERITUS
University of Illinois,
Chicago

JEREMY D. POPKIN. *Revolutionary News: The Press in France, 1789–1799*. (Bicentennial Reflections on the French Revolution.) Durham, N.C.: Duke University Press. 1990. Pp. xx, 217. Cloth \$32.50, paper \$8.95.

One would expect historians to be sensitive to the great impact of newspapers in the past, but Jeremy D. Popkin argues that until recently historians of the French revolution greatly underestimated the influence of the press. They tended to study the revolutionary newspaper less as a shaper of political life than as its mirror.

Like Pierre Rétat, Claude Labrousse, and a few other scholars, Popkin makes much grander claims for the press: "The events of 1789 to 1799 could not have occurred as they did and taken on the meaning that they assumed without it" (p. 180). He points out that the press promoted the spread of information, the exchange of ideas, the interpretation of events, and political consciousness. It helped to create a new political culture marked by popular sovereignty, by public discussion and participation, and by partisan politics that threatened as well as encouraged representative government.

This book is full of illuminating ideas supported by extensive research. Popkin proves his thesis while discussing informatively a broad range of topics concerning the revolutionary press: the journalists, publishers, and readers; the printing process, format and design, subscription prices, press runs, and profits; the coverage of typical newspapers of various political stripes; and the way in which the revolutionary press differed strikingly from that of the Old Regime and the Napoleonic period.

Occasionally Popkin makes claims about the role of

the press that cannot be fully substantiated in a short survey: "As the vehicle of the words and representations that made up revolutionary politics, the press was the great innovation that made the Revolution different from all earlier episodes in French history and thus opened the way to the modern political world" (p. 183). Generally his thesis is presented in a measured way with the necessary qualifications. He notes that only a minority of the French at that time read newspapers. The press hardly reported the story of the fall of the Bastille and was muzzled during parts of the Terror. Also journalists and publishers were constrained at other times in describing events as they saw them.

Popkin is well qualified to discuss the revolutionary press. He has edited with Jack R. Censer *Press and Politics in Pre-Revolutionary France* (1987) and has written two monographs, *The Right-Wing Press in France, 1792–1800* (1980) and *News and Politics in the Age of Revolution: Jean Luzac's Gazette de Leyde* (1989). Cognizant of the newest research techniques, he has mastered an impressive range of sources. These include newspapers of the revolution, relevant manuscripts, and secondary works in French, English, and German.

Popkin's latest book is also worth reading because it compares and contrasts the press of the revolution with the eighteenth-century British daily and the modern newspaper. Clearly and colorfully written, this book is an excellent contribution to the series *Bicentennial Reflections on the French Revolution*, edited by Keith M. Baker and Steven L. Kaplan, which provides a forum for new interpretations of the revolution to replace the previously dominant Marxist one.

FRANK A. KAFKER
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JEREMY D. POPKIN. *News and Politics in the Age of Revolution: Jean Luzac's Gazette de Leyde*. Ithaca: Cornell University Press. 1989. Pp. xii, 292. \$34.50.

This is a first-class study of the crucially important periodical *Gazette de Leyde* and of its extraordinary editor Jean Luzac, who provided through its pages some of the best, most astutely analytical political journalism of the late eighteenth century. Well-placed to gather both information and attitudes from other countries, and beyond the reach of censorship—which did not operate effectively outside France's borders—the *Gazette* was the freest, most candid, broadly international French-language newspaper of the prerevolutionary period. Jeremy D. Popkin argues persuasively that it maintained a special kind of superiority even after the revolution brought freedom of the press and hence a huge proliferation of papers onto the French market itself. Luzac, choosing his contributors and contents criti-

cally and judiciously, kept a kind of lucidity and distance that the various revolutionary journalists, caught in the passions of the events themselves, could not. He was devoted throughout his career to liberty, and never hesitated to expose what purported to be revolutionary freedom as merely another despotism. The *Gazette's* voice, denounced by radical leaders as counterrevolutionary, was one of the clearest, calmest, and most principled of the day. Luzac foresaw extremism in the early violence of 1789, praised the gradualist approach of the National Assembly, and tried to dispel panic over the king's flight even as he recognized its gravity; gave measured, highly original interpretations of events, documents, and behaviors; advocated peaceful means to reconcile opposing factions within France and even warring nations after 1792; predicted and detected rifts during the Year II; and knew before most others that the revolution would indeed devour its own.

More significant even than the outstanding reportage, war coverage, and news analysis in the *Gazette's* pages over its many years was the way in which it enabled discussion and debate about politics, and this long before the actual outbreak of the revolution. Popkin sees such debate as the paper's main contribution to the formation of common political culture during the prerevolution period. It had, as he points out, an "agenda-setting function," telling readers not what to think but what kinds of issues to think about (p. 253). Although the paper made no claims to represent public opinion, it certainly did its share to shape it and thus to guide events. In that sense it had a very modern understanding of journalistic purpose already during the Old Regime.

The book is well organized, well written, well argued. Anyone interested in eighteenth-century political culture would learn much from this scrupulously thorough study.

NINA RATTNER GELBART
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MARGARET H. DARROW. *Revolution in the House: Family, Class, and Inheritance in Southern France, 1775–1825*. Princeton: Princeton University Press. 1989. Pp. xiv, 279. \$39.50.

Margaret H. Darrow has written a nuanced study of the family, that most intimate social group, throughout the French revolution. She links revolutionary ideals regarding equality—and consequent inheritance law—with inheritance practices and family strategies. In 1793, after two years of revolutionary debates, the Convention passed laws eliminating primogeniture by decreeing equal inheritance among children; these were modified by the Napoleonic Code in 1804 that enabled testators to will an additional portion of the estate to an heir of their choice. The effect of these laws on French landholding

patterns and family viability long has been a subject of keen debate, and Darrow's account is the first cross-revolutionary study to assess their impact. This important investigation focuses on families in and around Montauban, which is an administrative, commercial, and manufacturing center just north of Toulouse. Montauban is of special interest to French historians because of its substantial Protestant minority and to family historians because of the prevalence of stem family households among the region's peasantry. The author's primary sources are wills and marriage contracts from the town's rich notarial records.

After an introduction to inheritance law, Montauban, and its inheritance patterns, Darrow investigates the pre- and postrevolutionary inheritance strategies of merchant, magistrate, artisan, poor urban, and peasant families. By so doing, she provides a finely drawn portrait of families in every circumstance. Darrow's lucid explanation of changing family situations reveals more than the book's title implies about family strategies and French society; along the way the reader learns, for example, about networks among Protestant merchants, elite women, and poor neighbors. Lively, apt, and memorable family stories illustrate the author's analyses of each social group.

Darrow found that the impact of revolutionary and Napoleonic legislation was muted by changes in the economy of Montauban, whose industry and commerce atrophied. In her close investigation of gender, she found that, although none of the new laws afforded protection for widows, families found ways to provide for them. New inheritance laws most affected the artisans, shopkeepers, and land-poor peasants, who divided their goods because they became too poor to gather a patrimony, whereas elite and landholding peasant families attempted to concentrate their patrimony in the hands of one heir despite equal inheritance laws.

Notarial documents are admirably suited to such an inquiry. Where they fall short, as in the investigation of the poor, Darrow looks to other sources such as hospital and court records. But because the study is so rich, it evokes fascinating questions that cannot be addressed by these sources; for example, one would like to understand the role of religious ideology in distinguishing the marital strategies of Old Regime Protestant merchant families (who married off their children) from Catholic families (who kept more children celibate).

This study offers a resounding confirmation of theories of the family economy and of family strategies. Darrow discerns strategies implicit in family behavior as well as fissures along gender and generational lines that ruptured family solidarity. Her most telling finding is that the nature of the patrimony (be it property, office, or simply a few linens and pots)

shaped the workings of family ties and distinguished degrees of parental power.

LESLIE PAGE MOCH
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COLIN JONES. *The Charitable Imperative: Hospitals and Nursing in Ancien Régime and Revolutionary France*. (The Wellcome Institute Series in the History of Medicine.) New York: Routledge. 1989. Pp. xi, 317. \$65.00.

Colin Jones's book is the latest in a burgeoning literature by recent French and English-speaking scholars who have addressed the living conditions and health of the poor and disadvantaged classes of premodern France. In this collection of eight essays, Jones looks at the institutionalized sick, including soldiers, prostitutes, and lunatics in the two thousand hospitals and asylums of France. By the revolution, they numbered 120,000, the largest hospital population in Europe. Jones's institutional case studies focus on Montpellier and Languedoc, already familiar to readers of his earlier work.

By "charitable imperative," the author refers to the combination of Christian charity and social obligation that characterized the treatment of the poor by French governments before the advent of the welfare state from the age of absolutism to the July Monarchy. Notwithstanding the antihospital biases of the Enlightenment and the wholesale dissolution of hospitals under the revolution, Jones sees continuity rather than transformation over two centuries of the desacralization of poverty. One-half of the national hospital population was housed in the *hôpitaux-généraux*, centers both of compassion and repression founded in the orthodoxy and discipline of post-Tridentine France. These were hospitals not in the medical sense but rather houses of reclusion and isolation, meant at once to remove from society and to succor the ailing, dependent, deviant, and dangerous classes of the realm. Jones sees ground to temper Michel Foucault's excessive judicial and repressive interpretation of these institutions that made major contributions to poor relief through bread doles and home care.

Jones also sees as exaggerated the "Black Legend" of Paris hospitals in the late eighteenth century, whose appalling hygiene and mortality were exploited by scientists, physicians, and social critics eager to see the hospital transformed from a dumping ground and deathtrap into a health factory of acute patient care and a center for the training of doctors. Although Jones furnishes evidence for dramatically reduced mortality in some hospitals of the Midi, he still finds the same deplorable conditions of crowding and infection as in Paris as well as progressive physicians who wanted to see the hospital become a clinic. The Midi physicians were equally opposed to

the internal ascendancy of religious nurses, who interposed their parochial authority in the entire hospital regime from admissions to therapy, medication, diet, and discharge.

Readers will welcome Jones's treatment of the hospital nurses, particularly the Daughters of Charity, founded by Saint Vincent de Paul and Saint Louise de Marillac in 1633. A group of nursing professionals with an elitist organization dedicated to the highest standards of discipline, order, and patient care, they became the largest and most familiar of the numerous communities of nursing sisters whose influence persisted into the second half of the nineteenth century. Jones's archival research in the contracts of the Daughters sheds light on the wide administrative, financial, and medical authority they exercised within the hospital.

In his essay on military medicine, the author correctly identifies military hospitals as the first medical centers dedicated neither to charity nor repression. By the eighteenth century they became models for the national dissemination of progressive clinical practice and hospital organization. The institutionalization of prostitutes furnishes another illustration of how the "charitable imperative" evolved from earlier moral-spiritual confinement to work and social integration. Similarly, the care of the insane exemplifies the transition from isolation and reclusion to medical treatment, pioneered by Philippe Pinel and Jean-Etienne-Dominique Esquirol, that culminated in the triumph of the psychiatric profession and the law in 1838 establishing a national network of lunatic asylums.

Jones has written a judicious and perspicuous account of the hospitalized classes of France. Rich sociological data together with a battery of painstaking maps, figures, and tables documenting the diverse inmate populations lend a vivid picture of their age, gender, provenance, ailments, professions, and condition. A summarizing conclusion would have strengthened the book as well as a bibliography of the prodigious source and monographic material cited by the author, which the reader is left to slug through in the endnotes of chapters.

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ANNE SA'ADAH. *The Shaping of Liberal Politics in Revolutionary France*. Princeton: Princeton University Press. 1990. Pp. xiv, 248. \$32.50.

Anne Sa'adah's study of the origins of French liberalism is a detailed examination of the ways in which the French revolution shaped that liberalism. Her book is also a comparison of the French revolution with the English and American revolutions. By narrowing her scope to three revolutionary processes relatively close to one another in time and in terms of

political, economic, and social structures and by focusing on a single question, Sa'adah avoids many of the problems of the large-scale comparative histories of revolution by Crane Brinton, Barrington Moore, Jr., John Dunn, and Theda Skocpol, among others.

The heart of the book is formed by two lengthy chapters on the French revolution. Chapter 2 reviews developments in the Old Regime, then covers the period 1789–92 in detail. The author's thesis is that the moderates in the French revolution never succeeded in developing an ideology that could serve as the basis for "a coherently motivated and systematically pursued plan of action" (p. 79). Ideology, or lack of it in this case, counts in politics. More than a set of ideas, it may provide a basis for responding to a particular political situation. Sa'adah contends that conditions in the Old Regime prevented the formation of a group in civil society that possessed enough independence to furnish a moderating force and an ideology that it might put to use. Stated differently, the Old Regime provided a "structure of revolutionary opportunity" (p. 82), but the power and reach of the state prevented a large, self-conscious moderating force from emerging. And it proved impossible to infuse old ideas and traditions with revolutionary meaning, as was the case in England and the United States.

Sa'adah's analysis of the period from 1789 to 1792 will be largely familiar to students of the French revolution. What makes it different and helpful is her ability to show how French political figures failed to establish a basis for a "politics of transaction" (p. 69). Unable to reach a consensus about how the political system should function, they could not end the revolution. Instead, the Girondins, "the great sorcerer's apprentices of the Revolution" (p. 132), turned to a "politics of exclusion" (p. 132).

The Jacobins were far more successful as practitioners of the politics of exclusion than the Girondins. Sa'adah postulates in chapter 3 that Jacobinism furnished, in effect, a second model of liberal politics, a model that emphasized fear of power. This fear and the emergency conditions that the revolutionaries found themselves in by 1793 reinforced a politics of exclusion that relied increasingly on the guillotine. The irony was that the Jacobin pursuit of the liberal goal of limiting governmental power had to result, in a relatively short time, in its undoing. By definition, it could not long support dictatorship. Nevertheless, this period of the revolution was long enough to establish a political pattern. It was impossible to return to the Old Regime. French liberalism, as the revolution shaped it, was dominant but characterized by instability and fragmentation. In the nineteenth century, a new element was added: Bonapartist interludes occurred as reactions to this model of liberalism.

This is a skillfully argued and valuable book. Based on careful reading of primary sources and a broad familiarity with relevant historical and political sci-

ence writings, it provides a useful perspective on the French revolution and illuminating comparisons with the English and American revolutions. Along the way, Sa'adah convincingly refutes J. L. Talmon's critique of Jacobinism in *The Origins of Totalitarian Democracy* (1970). Finally, Sa'adah has interesting suggestions to make about the patterns of French history since the French revolution, even if it is difficult to envision Charles de Gaulle as some sort of transitional figure from the politics of exclusion to the politics of transaction. Historians should welcome this excellent work of political and intellectual history, which just happens to have been written by a political scientist.

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SIMON SCHWARZFUCHS. *Du Juif à l'Israélite: Histoire d'une mutation, 1770-1870*. Paris: Fayard. 1989. Pp. 351. 120 fr.

The bicentennial of the French revolution stimulated the publication of numerous volumes of historical reflection, among them this synthetic history of French Jewry from the eve of the revolution until the loss of Alsace-Lorraine in the Franco-Prussian war. It was through the hesitant decision during the revolution in favor of emancipation that France's Jews became the first in Europe to acquire the rights and responsibilities of citizenship. Their early emancipation, along with Napoleon Bonaparte's establishment of an organizational framework for French Judaism, the consistory, shaped the process of acculturation and integration of the largely traditional and fragmented Jewish population of France.

Although the story that Simon Schwarzfuchs tells is a familiar one, he tells it well. Fully at home in the primary sources of the period, he is particularly skillful in depicting in considerable detail the different prerevolutionary Jewish communities in France as well as the complexities of the political process of emancipation during the revolution. Reflecting recent scholarship, he pays appropriate attention to the varied activist strategies of the Jewish communities of southwest France, Paris, and Alsace-Lorraine to advance their cause. Although aware of the ambivalence within revolutionary ranks about the desirable status for Jews in the new society, he rejects simplistic denials of the positive connotations that have been attached to the term "emancipation." His narrative reaffirms the centrality of the revolutionary act of emancipation in the creation of the legal preconditions for the Jewish confrontation with modernity. Offering a moderate and judicious reading of the different meanings attached to emancipation by various sectors of the French polity, Schwarzfuchs demonstrates how emancipation and its consequences led to the creation in the nineteenth century of a unified French Jewry from disparate pre-revolution-

ary Jewish populations. He is particularly sure-footed in analyzing the impact of the state recognized, centralized consistorial system, with its domination by lay notables, on the pace and nature of Jewish adaptation to nineteenth-century French culture. Equally persuasive is his brief assessment of French anti-Semitism and missionary efforts as of little significance in the period under study. Although the book's title erroneously suggests a straight movement from "Jew" to "Israelite," Schwarzfuchs incorporates recent scholarly findings that suggest the persistence of ethnic solidarity and identity among French Jews previously described merely as "assimilated." He points out, for example, that Jewish efforts at "regeneration," however successful in terms of acculturation and social mobility, did not achieve the goal of many proponents of emancipation—an economically and socially assimilated Jewish population differing from the larger society only in its religious affiliation.

The book is unbalanced, however, in its treatment of the century from 1770 to 1870. More than two-thirds of the text deals with the years until the end of the Napoleonic empire, with the chapters on Napoleon recycled from Schwarzfuchs's earlier English-language book on the subject. Because of the emphasis on the revolutionary and Napoleonic periods, the important social and ideological changes that occurred in the subsequent five decades receive little more than passing attention. Although such major issues as acculturation and social mobility, the nature of French-Jewish identity, and the establishment of the French-Jewish international defense agency, the *Alliance Israélite Universelle*, are touched on, none is explored in depth. Moreover, presentation of the programs of the consistorial elite and of the successes of prominent French-Jewish individuals is not adequately complemented by analysis of the behavior of ordinary French-Jewish citizens. Finally, because this book is presumably intended for general readers as well as specialists, its usefulness would have been enhanced by the inclusion of an index and by a more extensive bibliography.

For those interested in the impact of the French revolution on the development of French Jewry, this book provides a good introduction. Complementing the general histories of the modernization of Central and West European Jews that are German centered, this study can stimulate a comparative analysis of the transformation of European Jewry during the nineteenth century.

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MICHAEL P. HANAGAN. *Nascent Proletarians: Class Formation in Post-Revolutionary France*. (Studies in Social Discontinuity.) Cambridge, Mass.: Basil Blackwell. 1990. Pp. x, 243. \$34.95.

Labor historians increasingly recognize that proletarianization was more than a process of separating workers from skill and property. It was a diverse process involving the transformation of family life and political expectations. This study of three towns of miners and metalworkers near St. Etienne between 1840 and 1880 is a sophisticated explanation of the process.

A blend of skilled and temporary migrant laborers dominated the Stéphanais in the 1840s. During the revolution of 1848, this produced a politics characterized by demands for securing the traditional family economy (for example, farmland for children) and inconsistent class objectives (producer associations with small capitalists against "migrant" workers). More modern family and more thorough class politics emerged only after further industrialization.

Michael P. Hanagan traces the emerging market for a permanent proletariat with the gradual increase in the scale, capital, and technology of production. Employers established a rudimentary system of education and conditional welfare to assure the reproduction of a trained and tractable work force. In turn, workers reacted with their own family politics. Because the metalworker required formal education to obtain the skills necessary for high piece-rate earnings, he was attracted to worker-controlled and secular education for his children. The miner, by contrast, learned on the job. His concern was with income security (accident and old age insurance) to support his large family, produced when he was at his earning peak in his youth. Hanagan realizes, of course, the limitation of family politics and that proletarian households largely took care of themselves. Despite the emergence of a permanent proletariat of male household heads, families continued to rely on the wages of children and work of women to survive.

Yet it was these family-oriented issues, not concern over work organization as in 1848, that formed the political consciousness of these workers. The conflict between local and "foreign" workers in 1848 was reduced by changes in the migration patterns by the 1860s. Analysis of migration from rural "sending" areas reveals a trend beginning about 1860: because of changing opportunities in farming, seasonal and return migration from distant rural districts was gradually replaced with permanent settlement from closer communes. Hanagan also finds a decline of social segregation by area of origin in marriage, dialect, traditional holiday making, and other social patterns. Instead, geographically diverse boarding houses and bars emerged to facilitate the socialization of single male migrants in the Stéphanais. The result was a more unified working class than the one that existed in 1848.

The reemergence of political life in the mid-1860s allowed these demands for secular education and family welfare to be articulated. Hanagan argues that this led directly to the independent labor movement of the 1870s. He is suggestive, if somewhat specula-

tive, about the continuing linkage between family and work issues.

In a time when many social historians seem to have abandoned the toil of quantitative analysis, this work is a welcome confirmation of what social science methodology has yet to offer. Its efforts to link work, family, and politics in an analysis of the still elusive question of class formation is admirable. Particularly helpful is its revelation of the demographic and economic roots of socialist reformism. We all too often forget that this movement was largely about families and their survival in the industrial world.

GARY CROSS

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BENJAMIN R. GAMPTEL. *The Last Jews on Iberian Soil: Navarrese Jewry, 1479/1498*. Berkeley and Los Angeles: University of California Press, in cooperation with the Center for Medieval and Renaissance Studies. 1989. Pp. xi, 226. \$32.00.

In his first scholarly book, Benjamin R. Gampel has painstakingly gathered together snippets of information from notarial registers, church records, municipal receipts, and royal tax rolls and skillfully built them into the story of a tiny Jewry in northern Spain that survived the general expulsion of 1492 only to succumb almost totally to a forced conversion some six years later.

Gampel portrays a community of a few thousand Jews, at most 3.5 percent of the total population, concentrated primarily in urban centers such as Tudela and Pamplona and living in separate fortified *juderías* that afforded both convenience and protection without being hermetically sealed off from the rest of society. He stresses the Jews' economic integration into society: most made their living in various aspects of cloth manufacturing and trade, but some owned residential and even agricultural land, produced wine for their coreligionists, or imported the salted Atlantic fish that their Christian neighbors consumed during Lent. Highest on the economic ladder, above the jewelry makers and doctors, were several wealthy families that lent money, farmed taxes for various governmental authorities, and provided other financial services. Not surprisingly, these same wealthy families also dominated the *aljama* or Jewish communal government that both serviced and taxed its members with the crown's authority.

In May 1492, Ferdinand and Isabella published their decree of expulsion, and perhaps as many as two thousand Castilian and Aragonese Jews fled to Navarre. One of Gampel's best chapters deals with the fate of these refugees and of the strenuous local opposition to their admission. Apparently, many of the refugees found Navarre inhospitable and soon moved on. Others were so overwhelmed by events that they gave up their Jewish identities, converted to Christianity, and begged to be allowed back into their

old homes in Castile or Aragon. But many stayed and struggled to begin life anew, only to be caught by the Navarrese expulsion decree of 1498. Because none of the surrounding countries would grant the Jews visas, the decree became, de facto, a forced conversion, and only a very few had the courage and conviction to risk their lives by trying to escape to a Jewish community elsewhere.

Gampel's source material has dictated a sometimes dry and fragmentary presentation, but the details often suggest broader social realities that deserve further exploration. We wonder, for example, what to make of the fact that cemeteries were so crowded that bones had to be exhumed and stored in a common "charnel house" (p. 56). To the extent that his material allows him to generalize, Gampel stresses two complementary themes. First, Navarrese Jewry was still "demographically vital, economically vibrant and communally capable" on the eve of the expulsion (p. 68) and had good reason to trust in its own future. Second, the final expulsion came only because of outside pressure from Ferdinand and Isabella (pp. 129, 134).

But Gampel does not always have enough data to make this picture of continued *convivencia* stick. He himself cites strong evidence of broadly based religious hostility and mounting anti-Jewish pressure from the municipalities. To me, at least, it seems equally plausible to argue that these "last Jews on Iberian soil" were victims not of international politics but of the rising passion for religious purity that had put an end to medieval Jewries all over Western Europe.

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STEPHEN HALICZER. *Inquisition and Society in the Kingdom of Valencia, 1478–1834*. Berkeley and Los Angeles: University of California Press. 1990. Pp. viii, 444. \$45.00.

Like its Iberian counterparts, the Valencian Inquisition was a regional institution subservient to a national agenda. Neither monolithic nor independent beyond an elastic circumscription, it reported ultimately to Castilian power, characterized by a principled resistance to modernization but flexible in practice toward the incorporation of modernization under its control.

Stephen Haliczer's study joins the new approaches to the Inquisition, which, though differing among themselves, reject the persistent conceptualizations of the Inquisition as monolithic, peripheral, or, like Athene from Zeus, the brainchild of the Catholic sovereigns. With the fundamental assumption "that institutions created by a specific society and culture are so embedded in that society and culture as to be

inseparable from it" (p. 359), Haliczer unravels the complexity of the Inquisition and its sociopolitical symbiosis with its Valencian, Spanish, and general Iberian surroundings. Within a broad chronological framework, he structures his findings topically, studying successively the Valencian tribunal's origins and initial societal involvement; its procedures and income; its officials, from high to low, with welcome attention to the *familiares*; its religiously framed targets—New Christians, Moriscos, Illuminists, Erasmians, and Protestants; its post-Tridentine alterations; and its twilight and demise. Within each major rubric the presentation is again broadly chronological, richly detailed, and exemplified with case histories of individuals, families, and groups.

Haliczer's study is based, on the one hand, on meticulous archival research into the rich documentation of the Valencian Inquisition, more abundantly extant than those of many other local tribunals, though not without problems of retrieval and reconstruction, and voluminous genealogical, epistolary, and other records; and, on the other, on painstaking reconstruction from the solid foundation of contemporary sociopolitical understanding. This investigation enables Haliczer to provide comprehensive frames for the discrete activities of the Inquisition and cumulatively to demonstrate "the inexorable transformation of a once-alien institution into one much more closely identified with the Valencian scene" (p. 6). It enables him as well to deal convincingly with the shifts in power and purpose of the Inquisition before and after the Council of Trent, a watershed in its history as in the church at large.

The intense political involvement of the Inquisition in numerous spheres of Valencian and Spanish life emerges clearly from Haliczer's pages, as does its power over thought, not least in academic life, perceived as threatening by the then-regnant old guard. All of this resulted in the promotion of a "stultifying orthodoxy" (p. 329) and, as Haliczer felicitously puts it, "creating and maintaining the impression . . . that safety and preferment lay in treading the well-worn path of orthodoxy, however imprecise its definitions" (p. 325).

The depth of Haliczer's research and the breadth of his presentation create the need for, first, methodical analysis, reconstruction, where applicable, of all the extant cases before the Valencian Inquisition, and a multifaceted statistical study of their details and, second, sociopolitical analyses of the interaction of the spectra of central, regional, and national power with those of the Valencian Inquisition and, in general, the ecclesiastical arm.

Haliczer's study is presented with meticulous accuracy. If capable of enrichment by the consultation of some works in Hebrew, Portuguese, Spanish, and Dutch with illuminating parallels or engaging theoretical perspectives, the book is, as it stands, an

indispensable contribution to the study of Valencia, Spain, Iberia, and the modern Western world.

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RICHARD HERR. *Rural Change and Royal Finances in Spain at the End of the Old Regime*. Berkeley and Los Angeles: University of California Press. 1989. Pp. xxx, 879.

This massive volume of more than nine hundred pages is a rich, multidimensional analysis of changes in rural Spain in the last half of the eighteenth century. At one level, it is a study of royal and ministerial agricultural policy making, of reforms imposed from above to revitalize and increase Spanish farm production. At another level it provides a detailed micro-analysis of the changes in seven farm towns in the provinces of Salamanca and Jaén—geography, demographic makeup and evolution, occupational and class structure, income by class and activity, landowning patterns, types of economic activity, tax structure, market orientation, and the process and effects of the sale of disentailed lay and church property at the end of the eighteenth century (*desamortización*). The author also assesses agricultural developments in late eighteenth-century Spain within a Malthusian and Ricardian context, asks whether the epoch was characterized by a bourgeois revolution in agriculture, and evaluates the successes and failures of disentanglement.

From the 1760s, faced with a rising population, seemingly inefficient land use, food shortages, and higher food prices, royal ministers pursued a variety of policies to resolve Spain's agricultural crisis. Bureaucrats around Charles III saw the solution of the problem in populating "the barren wastes of Spain with homesteaders" (p. 68). Later royal reformers and political economists at the court of Charles IV, faced also by a need to save royal credit, believed that creation of a large class of small, independent farmers would cure Spain's agricultural and financial woes: hence, the calculated decision to sell off entailed ecclesiastical and lay estates. This policy, they hoped, would create a new group of enterprising, hardworking small landowners, bring unused or inefficiently farmed land into production, and resolve the monumental financial problems of the state.

Richard Herr's systematic analysis of the seven towns of Salamanca and Jaén and the two provinces overall reveals that sale of disentailed land had mixed results. Three types of buyers emerged to purchase the disentailed properties—big absentee owners who saw land as a good investment, local ecclesiastical and lay notables, and small enterprising peasants who worked the land. The effects of disentanglement on local areas depended on the quality of land, extent of

outside ownership, other economic alternatives available in the region, and demographic patterns. Most local farmers found themselves caught in both a Malthusian and Ricardian bind. As population rose, less land was available per capita, causing per capita farm income to diminish; for their part landowners charged higher rents to squeeze the local farmer even more. Yet Herr believes that disentanglement—30 percent of the land changed hands in Castile—allowed some enterprising peasants to increase their landholdings, which sharpened socioeconomic differences in some regions. On balance, the author feels that disentanglement was beneficial, hurrying the pace of much-needed rural change in Spain by fomenting the commercialization of agriculture. He disputes the conventional wisdom that disentanglement allowed large plots of land to fall into the hands of greedy speculators, who charged outlandish rents on their land; he also discounts the view that disentanglement contributed to a bourgeois agricultural revolution. But if *desamortización* was a partial success in encouraging agricultural change, it was a failure in resolving the government's fiscal crisis; it did not save the royal credit.

This book entailed an enormous amount of research. From parish tithe rolls, detailed census listings (*catastros*), notarial documents, deeds of deposits, records from royal bureaus established to deal with disentanglement, royal laws, and a host of other local and pan-regional sources, Herr has re-created the reality of rural Spain from both a local and a national vantage point and is equally at home with the local farmers of Baños, La Mata, Lopera, and Villaverde and the ministers sitting around royal council tables in Madrid. Methodologically, he meticulously casts his analysis into a rigorous comparative framework (for example, using equivalent fanegas in wheat [EFW] as a common income unit for the different towns and villages) and significantly reinforces his narrative with over two hundred tables, twenty-five maps, twelve plates, thirty-eight beautifully wrought graphs and figures, and almost one hundred pages of appendixes. Some may find that Herr tells them more than they want to know about rural Spain at the end of the eighteenth century; others, however, will be well rewarded by reading this richly textured book.

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RENATO BARAHONA. *Vizcaya on the Eve of Carlism: Politics and Society, 1800–1833*. (The Basque Series.) Reno: University of Nevada Press. 1989. Pp. xvii, 328. \$34.95.

Throughout the nineteenth century and continuing to the Civil War period (1936–39), revolutionary and counterrevolutionary movements occurred with such regularity in Spain that they became an accepted part of the national political scene. Certainly one of the

most persistent and, indeed, quintessentially Spanish of these was Carlism, an ultraconservative cause that was officially inaugurated by a war in 1833 and did not completely die out until the advent of the Franco dictatorship (1939–75). Thanks largely to scholars such as Martin Blinkhorn, José Carlos Clemente, John Coverdale, and, most recently, Renato Barahona, the place this once powerful and influential movement holds in Spanish history has been firmly established. In contrast to previous scholars, who have tended to explain Carlism in terms of its overall ideological aims and political program, Barahona has written exclusively about the social, political, and economic conditions that gave rise to Carlism in the Basque province of Vizcaya, a region where the movement threw down some of its deepest and most vibrant roots.

Although formally grounded on a legal claim to the Spanish throne—asserting that Don Carlos, the brother of Ferdinand VII, was the true heir to the crown—the Carlist cause was fundamentally a reactionary response to the modernizing efforts of Spanish liberal reformers. Above all, the Carlists were staunch defenders of a conservative Catholic church and the traditional economic and social institutions that were legacies of the *ancien régime*. Carlism was not in theory linked to the idea of separatism, yet its program recognized the medieval *fueros* (special rights, laws, and privileges) as the legal basis for the self-administration of the Spanish regions. Not surprisingly, therefore, the movement attracted a mass following in the Basque country and other areas where regional sentiments ran deep.

In Vizcaya a well-developed and pervasive sense of regional identity had long been a source of strained relations between the province and the central government in Madrid. Focusing on the years when these tensions were coming to a head, especially the period known as the “Ominous Decade” (1823–33), Barahona’s microhistory examines the socioeconomic dimensions of the power struggle that was waged between Vizcayans who were intent on conserving the long-standing and harmonious pattern of life peculiar to the region and the liberal constitutionalist elements who were seeking to transform it.

In delineating the boundaries of this conflict, Barahona brings to light the fact that, even though the lines separating the two opposing forces were clearly drawn, each camp was composed of a complex mixture of social classes. On one side stood the conservative Vizcayans, a group that included the clergy, landless rural classes, artisans, middle and small landowners, and even lawyers. Ranged against them were members of the landed gentry and particularly the urban-based merchants and capitalists, the class that would gain the most from the introduction of liberal economic policies.

Based on an impressive array of unpublished materials, many of them gleaned from local records and the French and Spanish national archives, Barahona’s

monograph offers a detailed description of the process by which the central government’s attempts to consolidate its control over a politically and economically fragmented nation inevitably undermined the foundations of a traditional Vizcayan society. Yet, as Barahona’s researches clearly demonstrate, the Vizcayan conservatives did not relinquish their local autonomy without a fight. For example, his chapter on the Vizcayan police and paramilitary units known as *Brigadas de Paisanos Armadas* provides a valuable account of how the traditionalists increasingly relied on force to check the growth of liberalism and, in so doing, paved the way for Carlist rebellion.

Because this study generally offers a vivid picture of a hitherto neglected aspect of the Carlist movement, it is a pity that many key sections are not written with nonspecialists in mind. The author could have easily extended the potential reading audience, for example, by providing a glossary of Spanish expressions, especially considering that he fails to define adequately in the text itself some significant terms, such as *afrancesados* (Spaniards who, following the French, sought to modernize Spain) and *apostólicos* (ultraclerical precursors of Carlism). And, because the author only cursorily deals with the theoretical connections between Vizcayan conservatism and Carlist doctrine, the reader may not gain a sense of the vital role ideology played in animating and sustaining Carlist support among the Basques.

Notwithstanding these minor flaws, Barahona’s solidly researched monograph is written in a clear prose, which is enlivened by the author’s obvious enthusiasm for the subject. His book makes an important contribution to our understanding of a fascinating historical phenomenon, which should encourage further interest in the social history of similar movements in Spain and elsewhere in Europe.

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IGNACIO ARANA PEREZ. *La Liga vizcaína de productores y la política económica de la restauración, 1894–1914: Relaciones entre el empresariado y el poder político*. (Biblioteca de historia del pueblo vasco, number 6.) Bilbao: Caja de Ahorros Vizcaína. 1988. Pp. 726.

The term “restoration” in modern Spanish history can refer to Ferdinand VII (1814–33), Alfonso XII (1876–85), or, as in this work by Ignacio Arana Pérez, to the regime of Alfonso XIII (1902–31). All three restorations failed to solve a series of difficult institutional and economic problems, but the spectacular failure of Alfonso XIII’s restoration to stabilize political life led directly to the chaos of the Second Republic, Spanish Civil War, and Francisco Franco. To analyze this era, Arana pays special attention to the continuity of patronage groups, rival elite pressures, and economic problems inherited from the

earlier restorations. The continuation of economic oligarchy, he concludes, kept alive unreformed conservatism and permitted Alfonso XIII to ignore reform in the early twentieth century.

The restorations of Alfonso XII and Alfonso XIII benefited from the Machiavellianism of two political leaders, Antonio Cánovas del Castillo and Práxedes Mateo Sagasta, whose efforts to pacify ultramontane Carlists with a new form of conservative liberalism succeeded only in increasing corruption and exaggerating the oligarchical tendencies of the *cacique* system. They ruled as "chiefs" of two political tribes who differed only in cosmetic ways; whenever one encountered difficulty, the other took power to bury the scandal. The Spanish American War revealed how corrupt this system had become.

In the postbellum period, the Generation of 1898 blamed the traditionalism of Spanish economic thought as an important element of the "disaster" by hampering Spain's ability to modernize and Europeanize. Recently, the argument has evolved into one of the more important historiographical issues in modern Spanish history. There have been contentious quarrels over free trade versus protectionism—Castile versus Catalonia—(J. Vicens Vives and J. Varela Ortega), the impact of agrarian crisis (R. Tamames, J. Nadal, and G. Tortella), or the slowness of railway construction to create national markets (Nadal). Gradually, however, a consensus position has emerged to argue that an overpowering triangular pact of economic oligarchs—Basque metal and mining interests, Castilian wheat producers, and Catalan manufacturers—rose to dominate national politics in the 1880s and 1890s to such an extent that their traditional values and special interests offset social change implicit in a modern economy and continued the spirit of Cánovas and Sagasta well into the twentieth century.

Arana provides a good history of the organization that institutionalized this oligarchy. The *Liga Vizcaína de Productores* was created in 1893 as a forum to discuss Asturian and Basque economic questions. Its history provides general insight into the elites of northern Spain and particularly into their support for economic nationalism after the loss of Spain's last colonies in 1898. Earlier British and French pressure had prevented adoption of protectionist tariffs, but, when the Spanish government sought to lure Germany as a new diplomatic partner by proposing favorable tariffs for German goods, a wide coalition of regional economic elites led by the *Liga* blocked it. Subsequently, the oligarchy increased its political involvement enormously, and new campaigns to increase tariffs or weaken special rights of foreign investors dominated the period down to 1914. This new elite power and cohesiveness repaired the damage to conservative liberalism caused by the Spanish American War and allowed the increasingly more authoritarian regime to stay in power for an additional several decades.

Arana does not always make clear the political consequences of these events, and linkages to major national politicians or lobbying activities in Madrid also are neglected. Still, the economic and social history of the *Liga* is an interesting chapter in early twentieth-century Spanish politics.

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ROCKWELL GRAY. *The Imperative of Modernity: An Intellectual Biography of José Ortega y Gasset*. Berkeley and Los Angeles: University of California Press. 1989. Pp. xiii, 424. \$29.95.

In October 1982, the Hispanic Division of the Library of Congress and the Fundación Ortega y Gasset sponsored an international symposium for the dual purpose of announcing the library's recent acquisition of José Ortega y Gasset's unedited papers and other archival material from the foundation and of honoring the centennial celebration of Ortega's birth. Ortega was born in 1883. The centennial celebration marked the occasion with a series of international symposia and colloquia held in the United States, Spain, and Latin America between 1982 and 1985, most of which resulted in a resurgence of published essays and books on Ortega. Rockwell Gray's work has been published at the conclusion of the appearance of these studies and, apparently, has profited from their respective findings.

A central concern of the book is Ortega's commitment to the modernization of Spanish culture and thought. In his account of "Ortega's notion of modernity," however, Gray confuses the categories of "modern era," in the sense of the post-Renaissance period, and "modern philosophy," in the understanding of thought since Galileo and Descartes (pp. 14–23, 31–43). Such an obvious obfuscation of categories hinders Gray's attempt to explicate Ortega's "vision of modernity." In *The Theme of Our Time*, Ortega at once extolled and criticized the notion of "modern." Some attempt to distinguish this tendency becomes especially important in view of the fact that one of Ortega's main points in the work entailed his idea that "the theme of our time" (the twentieth century) had to extend beyond the "modern theme" (the nineteenth century). The absence of any distinction of this tendency has resulted in obscuring not only Ortega's "vision of modernity" but also the crucial differences between the categories "modernity" and "modernization."

After the defeat of Spain in the Spanish American War, the "generation of 1898," a circle of academics and intellectuals, reassessed the "national problem" through concerted efforts to modernize Spain. The issue of modernization became a crucial response to the perceived national problem for several young academics and intellectuals, among whom was Ortega. Gray's discussion of Ortega's characterization of

"avant-garde art" in the 1920s, however, eventually addresses more directly important issues inherent in the general understanding of the notion "modernity" than does Gray's discussion of the "generation of 1898" and the "generation of 1914." This characterization assumes the form of distinguishing traditional poetry from vanguard poetic expression, art of the "past" from the "new art" of the present (pp. 152–57).

Ortega's philosophy developed within phenomenology, the philosophical tradition in which Martin Heidegger and Max Scheler produced great works. Thus, one finds an inclination in various interpretations of Ortega's philosophy to focus on his concern with his own originality, on the priority of his ideas, and on demonstrating how earlier ideas made known by Wilhelm Dilthey, Edmund Husserl, Heidegger, and Scheler were anticipated by him. As a consequence of these concerns in Ortega, Gray continues a trend, which has evolved in scholarship about Ortega, in which commentaries follow the lead of Ortega and his disciple, Julián Marías, and point to the *Meditations on Quixote* (1914) as having formulated philosophical ideas in existential phenomenology that were expounded later by Heidegger in *Being and Time* (1927). A closer reading of Ortega's works, however, will provide evidence that points in a different direction. For, despite his subsequent claims of "chronological priority," Ortega in *What Is Philosophy?* (1929) aligned his maturing philosophical position with Heidegger's "new philosophy" of "being" and acknowledged how he and other philosophers "took notice" of the "enormous significance" of Heidegger's words: "To live is to find oneself in the world."

Gray's intellectual biography provides a lucid portrait of Ortega, the philosopher, as an educator who strove to bridge the gap between Spain and the grand traditions of European thought and culture. Gray's extended purpose, of connecting Ortega's "philosophical development" to his "political activism," is less successful than his purpose of linking Ortega's philosophic objective of clarity to his pedagogic mission of disseminating European thought and culture to the Spanish public. A penetrating discussion of Ortega's philosophical development requires more of an analytical interpretation of his thought than Gray's expository narrative approach permits. Whatever his approach in interpreting Ortega's educational purpose, philosophy, and politics, Gray demonstrates convincingly that despite the cultural and historical influences that Ortega received from abroad, Ortega transformed these external influences into a "perspective" that became his own distinctive brand of philosophy.

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JONATHAN I. ISRAEL. *Dutch Primacy in World Trade, 1585–1740*. New York: Clarendon Press of Oxford University Press. 1989. Pp. xxi, 462. \$72.00.

Historians have traditionally traced Dutch primacy in world trade to the skills developed in transporting cargoes of salt, fish, grain, and other bulky goods to and from the Baltic. Jonathan I. Israel agrees that the bulk trade of the Baltic brought the northern Netherlands to the brink of its trade supremacy by 1590. Its continued success, however, resulted from a shift to the "rich trades" in textiles, spices, and other high-value goods.

From 1590 to 1609, the Dutch catapulted themselves into world trade supremacy, aided by the wealth and expertise of immigrants from Antwerp, Rouen, and Iberia, many of whom were converted Jews. Silver acquired through trade with Spain, Portugal, and their colonies provided the crucial link joining various parts of the Dutch trading system. Consequently, Spanish wartime embargoes seriously hurt the Dutch and spurred them to seek direct links to the Levant and western Africa, as well as to the Western Hemisphere and the Far East.

During the Twelve Years' Truce with Spain (1609–21), most parts of the Dutch trading system flourished. Dutch merchants dealt with the Iberian world through official monopolies and made great gains in the Baltic, Africa, and the Far East as well, financed by American silver. The Dutch system reached its first peak in about 1620, based primarily on development of the "rich trades" in luxury goods from around the world and on the finishing industries developed at home for cloth and other goods. A serious crisis followed between 1621 and 1647, caused—according to Israel—primarily by the renewal of warfare with Spain, an interpretation that sets him against Fernand Braudel, Pierre Chaunu, and many other modern historians. Israel argues that Spanish trade embargoes had an immense effect on Dutch commerce because of the centrality of Iberian markets and silver to Dutch world trade. Only by desperate and imaginative adaptation were they able to keep their trading system afloat, often at high cost in military expenditures.

Once the Dutch Republic and Spain made peace, the Dutch system experienced its greatest prosperity and influence from 1647 to 1672, rather than beginning a decline around 1650 as many have argued. Dutch merchants reestablished their dominant position in trade with the Iberian world and the Mediterranean, based on luxury goods such as fine woolen cloth made from Spanish wool. Their bulk trade to the Baltic experienced some decline, but overall the four "great rich trades" centered on Cadiz, Smyrna, Batavia, and Archangel flourished as never before, connected by silver from America and spices from the Far East. England caused considerable damage to Dutch shipping in several trade wars, but Israel argues that strife with Spain (before 1648) and France (after 1672) had more serious effects.

Dutch primacy in world trade lasted only as long as its larger neighbors were willing to permit it. From 1672 to 1700 France, Sweden, and England put

increasing pressure on Dutch trade in Europe and around the globe. Alarmed by French ambitions, Spain allowed the Dutch continued access to trade and silver, bolstering their position in the Far East. Elsewhere, the Dutch were clearly past their prime by 1700. Between 1700 and 1713, they lost access to the Hispanic world by fighting against the accession of a French Bourbon to the Spanish throne. The Dutch world system disintegrated from 1713 to 1740, although some individual trade routes remained profitable thereafter, thanks to the general expansion of global trade in the late eighteenth century.

Israel's argument is carefully documented throughout, relying on a wealth of primary sources and a continuing critique of earlier work. Many of his conclusions rest on new calculations from well-known trade statistics; historians with a vested interest in their own calculations are not likely to agree with him. In addition, Israel tends to accept too readily the connection between politics and trade and unduly neglects everything but industry in considering the links between trade and the internal Dutch economy. Overall, however, he provides a persuasive corrective to earlier interpretations.

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ANSSI HALMESVIRTA. *The British Conception of the Finnish "Race," Nation, and Culture, 1760–1918*. (Studia Historica, number 34.) Helsinki: Suomen Historiallinen Seura. 1990. Pp. 324.

Anssi Halmesvirta has written a study of the development of British ideas about Finland and the Finns and the political attitudes that these ideas generated. He has excluded the governmental level and based the study on literary sources, mainly periodical, newspaper, and pamphlet literature and travelers' tales. The account starts in the eighteenth century, when British observers had finally differentiated the Finns from the Lapps on one side and the Swedish settler population on the other and identified Finns and their language as of Finno-Ugrian origin. The Finns were lucky, for the earliest British commentators rated them as certainly backward by comparison with the Swedes but capable of improvement. This was crucial for the future, because, although the Finns were assigned an inferior "Mongol" racial origin, they were also judged capable of transcending this misfortune and of rising to the level of civilization of more fortunate peoples. This was the view of the great traveler Archdeacon William Coxe, who journeyed in Russia and compared the progressive Finns with the stagnant and barbarous Russian peasants. The early scientific anthropologist, James Prichard, writing in the 1830s, rejected the racial determinism of other early anthropologists in favor of the adaptability of peoples to their environments. He pronounced that

not only had the Finns taken "some steps towards a progressive civilization" (p. 98) but in time might also emulate the historic achievements of the British themselves and become a liberal, self-governing community worthy to join advanced peoples.

As the analysis develops, it is striking how, right down to 1914, the thinking of British observers, most of them liberal or progressive, was rooted in racial ideas. There was a farrago of nonsense about supposed racial characteristics, skull sizes, hair color, and skin textures, which were supposed to identify races as superior or inferior, and these were reinforced by Darwinism as a basis on which peoples were to be classified. The classification was supported by cultural anthropology. In the case of Finland, British interest in the "Kalevela" poetry led to debates about the level of primitive Finnish culture and where it stood in the hierarchy of peoples and whether Finnish was, or could become, a "cultural" language.

The book shows clearly how the debates were largely conducted by people who constantly exposed in what they wrote their profound ignorance of Finland and its language. Nevertheless, as the Finnish nationalist movement developed in the nineteenth century, it evoked a mainly positive response in the British, particularly as British liberal intellectuals tended to be anti-Russian. The conflicts that developed in Finland after the 1890s between the nationalists and the tsarist regime could be seen as a struggle of civilized Finns against barbarous and reactionary Russians.

Halmesvirta has produced a competent and professional analysis of what is, admittedly, a somewhat marginal aspect of British cultural history, and he has done so with admirable objectivity in the face of the offensively patronizing attitudes of these British commentators toward his compatriots. It is well to be reminded of how deeply racist assumptions of superiority over foreign peoples permeated the thinking of British liberals and progressives in the great age of British imperial hegemony, even though in this case it had mostly benign consequences for Finland and the Finns.

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JOHN T. FLINT. *Historical Role Analysis in the Study of Religious Change: Mass Educational Development in Norway, 1740–1891*. (The Arnold and Caroline Rose Monograph Series of the American Sociological Association.) New York: Cambridge University Press. 1990. Pp. xv, 148.

John T. Flint's scholarly study in historical sociology is a welcome addition to the expanding body of historical literature about the Scandinavian states that is now starting to appear in the English language.

This study seeks to explore the relationship between educational development and religious change

in Norwegian society between 1740 and 1891. The Norwegian environment provides a unique context for this investigation. Unlike the diversity that characterized the practice of religion in the British Isles, Holland, and the United States during this same period, the people of Norway remained overwhelmingly Lutheran. As late as 1891, less than 2 percent of the total population of Norway could be identified as being outside of the state church. Religious protest movements in Norway, as a result, remained within the established order.

The author of this account uses what he calls "historical role analysis" to describe the changing relationship among state church pastors, parish school teachers, pupils, parents, and lay preachers. His attention focuses on the progressive laicization of religious leadership in the context of an environment where the definition of religiosity was changing from ritualistic to experiential requirements of a highly personal nature.

The process of laicization, which Flint describes, was fostered in Pietist Conventicals, Haugean circles, Inner and Outer Mission locals, and some separatist groups. Regardless of the context, its impact on Norwegian society was of immense importance because it produced and reinforced a continual need for functional literacy. Lutheran Pietism came to be the single most crucial force for initiating and sustaining literacy in Norway.

A wide variety of primary sources and secondary works have been examined in the research that led to the publication of this book. Census materials, reports to the Ministry of Church and Education, and organizational histories have been studied. The University Library in Oslo, the Norwegian Central Statistical Bureau, the Royal Archives, the library at the University Theological Faculty, and the Institute for Pedagogy all provided valuable materials. The author also acknowledges his indebtedness to earlier scholars such as Dogfinn Mannsaaker, Hans Jorgen Dokka, Stein Rokkan, Ingrid Semmingsen, Seimen Skoppel, and Sverre Steen.

The text of this work is further enhanced by a number of maps, charts, and tables. They are clearly executed, carefully referenced, and skillfully integrated into the text. Chapter reference notes and an extensive bibliography are located at the end of the book.

The annals of Western historiography are filled with accounts of denominational and sectarian rivalries. In many instances, these rivalries played an important role in advancing the cause of social, economic, and political reform. Norway, because of the homogeneous character of its religious environment, provides a fascinating context for such a study. Flint's contribution to the literature on this subject is significant. His book should be of special interest to sociologists of educational change as well as scholars with an interest in Scandinavian history and eight-

eenth- and nineteenth-century European reform movements.

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INGRID MILLBOURN. *"Rätt till maklighet": Om den svenska socialdemokratins lärprocess, 1885–1902* ["Right to Leisure": On Swedish Social Democracy as a Learning Process, 1885–1902]. Stockholm: Symposion Bokförlag. 1990. Pp. 438. 220 KR.

Maklighet can mean either laziness or quality of life, and here it signifies quality of life. The social democratic *maklighet* was a dream that the worker's life should be a utopia characterized by centralized, state-owned production, large scale operations, effective work methods, order, and discipline. In this book, Ingrid Millbourn concentrates on the period of social democratic childhood, 1885–1902, when the early theorists and planners were developing, testing, and expanding the program of Swedish labor and debating among themselves. The thoughtful leaders in the beginning stages were August Palm, a radical tailor, Knut Wiksell, an economist, Axel Danielsson, a politician and journalist, and Hjalmar Branting, soon to stand forth as greatest of them all. From outside Sweden, Karl Kautsky, Friedrich Engels, and Karl Marx came into this essentially non-national group.

Basically, this book is an idea history of Swedish Social Democracy in the generation of its origins. Like the American Founding Fathers, these Swedish innovators dreamed of a new society with new principles and new institutions. They envisioned a materialistic and equalitarian utopia. The programmatic ideals were achieved through intensity of effort, lengthy argumentation, and constant adjustment.

Emphasis was on productivity and therefore large scale manufacture, and this assumed, in the minds of the founders, state ownership and direction. Productivity was a factor not only of size but of effectiveness. The leaders of the labor cohort were aware of all of the multiple angles of hourly expenditure of working time, energy, degree of satisfaction in work, family concerns, and wages. Labor leaders everywhere read the reports of like-minded reforms in the United States, England, France, and other countries. If they learned, for example, that the eight-hour day seemed to give the best result in productivity combined with energy expended for mind and body, then that should become the norm for the work day and for the day's pay. The leaders of labor thus learned how to make reasonable demands of employers.

The learning process went beyond matters of wages and hours. Working conditions were important. Cooperation between labor and capital was seen as both logical and necessary, and a return to competition was a threat to the workers. The methods of the capitalists were viewed as successful and worthy of imitation. The catastrophe theory was abandoned; it

had never been wholly adopted. Several of the socialist leaders had never espoused it. The chief socialist propagandists soon joined hands in the cooperative movement, and the two grew together. One striking fact of this early period was that the leaders were men, and a significant number came from the middle class. Branting was the outstanding figure. The idea that the workers lacked influence was soon dispelled. Class consciousness was obvious, but the inevitability of class conflict was not universally accepted. The Marx-Engels-Kautsky group emphasized conflict more than the Swedish contingent, who in the beginning favored adjustment. Planning of production was considered to belong in the hands of the capitalists, who were thought to have handled it well.

Both the new and the old are understandingly treated in this study, and the social democratic infancy is sympathetically introduced.

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THORLEIFUR FRIDRIKSSON. *Den gyldne flue: De skandinaviske socialdemokratiens relationer til den islandske arbejderbevægelse 1916–1956; Internationalisme eller indblanding?* [The Golden Fly: The Scandinavian Social Democracies Relations to the Icelandic Labor Movement, 1916–1956; Internationalism or Intervention?] Copenhagen: Selskabet til Forskning i Arbejderbevægelsens Historie. 1990. Pp. 368.

Iceland's Social Democrats were once accused of taking money and advice coming to them surreptitiously from across the sea, as if on the wings of a "golden fly." Thorleifur Fridriksson has put this metaphoric insect under the microscope, and in this book he confirms that some Icelandic Social Democratic leaders did indeed both solicit and receive financial aid from Social Democratic comrades in Denmark, Norway, and Sweden. The grants and loans received over thirty-six years amounted to perhaps \$125,000. In the course of telling about this not overly generous aid, Fridriksson also tells a great deal about the Icelandic Labor Movement's history from 1916 to the mid-1950s. Originally the movement tried to be both a union and a political party wrapped into one, but a reorganization in 1940 made a clearer differentiation of the two. Thereafter party leadership and union leadership could differ on policies and often did so. Fridriksson tells a story of internal strife leading up to a spectacular crisis in the mid-1950s that featured a general strike. The contending factions were represented in part by mostly moderate Social Democratic party MPs led by Stefan Stefansson, on the one hand, and more radical, union-based party leaders led by Hannibal Valdimarsson on the other. In a pattern familiar to anyone acquainted with postwar European labor politics, the moderates viewed the radicals as zealous class warriors insensi-

tive to democracy's rules, whereas the radicals viewed the moderates as selling out to capitalist interests.

Fridriksson wishes to be objective and pronounces no judgment on which side was right, but in telling the story he does not hide the fact that he goes along with a common Icelandic view that taking foreign money for domestic purposes is a betrayal of Iceland or that he goes along with a common socialist view that for a socialist MP to deign to cooperate with a right-wing party instead of leftists is a betrayal of the workers. That makes the Social Democratic leaders who did both of these things look doubly treacherous, although the author does not come straight out and say so.

Fridriksson has dug deeply into archives and people's memories in Iceland as well as in Denmark, Norway, and Sweden to produce an account that is richly informative and thoroughly documented. The only thing that is missing is a grasp of political realities. He seems to think that those Social Democrats in the time of the cold war who opposed leftists in their ranks were mostly venal or irrational. He is blind to the possibility that anyone on the left might honestly have thought democracy needed defending against the extreme Left. Fridriksson is too quick to call any opposition to the extreme left "McCarthyism." He calls Haakon Lie, the shrewd executive secretary of the Norwegian Labor Party, which extended aid to the Icelandic moderates, "one of McCarthyism's most important proponents in Norway" (p. 254). This is absurd. Invoking McCarthyism as an explanatory device for the activities under study rather than seeing them as consequences of rational choices shows a failure to become detached from the partisan polemics of the times.

The book contains a valuable survey of scholarly literature on labor movements and Social Democratic parties in all of the Scandinavian languages. A fourteen-page summary in English is included.

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HELÉNE LÖÖW. *Hakkorset och Wasakärven: En studie av nationalsocialismen i Sverige, 1924–1950* [Hakkorset and Wasakärven: A Study of National Socialism in Sweden, 1924–1950]. (Avhandling från Historiska Institutionen i Göteborg, number 2.) Summary in English. Göteborg: Historiska Institutionen. 1990. Pp. 527.

This five-hundred-page dissertation will tell any reasonable person (who can read Swedish) everything he or she ever wanted to know about National Socialism in Sweden—and a great deal more. Those who cannot read Swedish are limited to a seventeen-page summary that is alleged to be written in English.

The book is exhaustive in its compilation of facts about the Nazi movement in Sweden. The complex history of the various parties and organizations that

made up the movement is presented in great detail. In the province of Bohuslan, which has been selected for special study, the work approaches the microscopic level. Similarly, the organization, the finances, the propaganda techniques, and the memberships of the various parties have been exhaustively studied. In addition, there is an interesting and (naturally) detailed recounting of the attitudes and policies of the governmental and police authorities vis-à-vis the Swedish Nazis. No one will ever need to bother with any of these topics again.

Of the two, in my opinion, really interesting questions concerning Nazism in Sweden, Heléne Lööw largely ignores one but does shed considerable light on the other. The first of these questions is why National Socialism failed to become more than a fringe movement in Sweden. Lööw adds little or nothing to our understanding of this problem. The other question of interest deals with the social composition of the Nazi and other far Right parties in Sweden. More specifically, how accurate are the charges made by the Swedish Communists and, more important, by the Swedish Social Democrats that the Swedish upper and upper-middle classes (social group I in Swedish terminology) were riddled with Nazis and Nazi sympathizers before and during World War II?

Lööw helps to answer this question by providing data on the social background both of party members in general and of party officials. There is also some useful information on the list of individuals the police considered to be (Nazi) security risks during the war. On the whole, the data do not lend much credence to the notion that National Socialism in Sweden was largely an upper-class phenomena, much less that it was embraced by a large percentage of the upper and upper-middle classes. The closest thing to confirmation of such an assertion is the relatively large number of military officers, especially high ranking officers, suspected of Nazi leanings in 1940 (p. 423). At least I, however, know of no evidence to indicate that Nazi sympathies among the officer corps were a threat to Sweden's ability and willingness to resist a German invasion.

Although the book adds to our knowledge of the social composition of Swedish National Socialism, the exposition in this area could have been improved considerably by the inclusion of appropriate comparative data. Thus, in addition to information on the social composition of the Nazi movement, the reader would have benefited from data on the social composition of the entire Swedish population and, even more important, on the social composition of the other political parties. Even the data on the extent of Nazi influence in the armed forces are made less enlightening by the failure to provide data on the size of the entire officer corps.

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ODDVAR K. HOIDAL. *Quisling: A Study in Treason*. Oslo: Norwegian University Press; distributed by Oxford University Press, New York. 1989. Pp. 913. \$55.00.

This book of some nine hundred pages, including notes and bibliography, is a good deal more than the title suggests. It is a massive survey of Norway's political evolution before and during World War II. The role of Vidkun Quisling is woven into the larger matrix of German occupation policies and Norwegian resistance efforts. From birth in a Norwegian parsonage to death against a wall in the Akershus Castle, the Quisling story is told and retold, although not a great deal emerges that is new. Many years ago Gabriel Langfeldt wrote a short psychiatric study of Quisling entitled *Gåten Vidkun Quisling* (The Vidkun Quisling Puzzle). Whatever its other merits, the volume by Oddvar K. Hoidal leaves the reader with the distinct impression that the puzzle persists.

This large book represents an extensive distillation of what Hoidal has found in the documents and writings of the time and what other Norwegian and foreign scholars have either discovered or postulated during the past forty years. Various documentary material has been dealt with prudently; contemporary memoirs have been used with care. Relevant newspapers and periodicals of the time have been examined, and all of the pertinent secondary writings have been used generously, particularly the pioneer work of such scholars as Ole K. Grimnes, Sverre Hartmann, Berit Nøkleby, Magne Skodvin, Sverre Steen, and many others.

In respect to the central figure in this book, Hoidal provides considerable biographical data. As a boy, Quisling distinguished himself as a brilliant student, and, despite a taciturn and retiring personality, his achievements in the classroom gave him unique status among his schoolmates, something that continued during his years at the War College. He spent a short time as a junior member of the General Staff, after which he was appointed military attaché to postrevolutionary Russia and after a few months legation secretary in Helsinki. For much of the balance of the 1920s Quisling served with one or another of Fridtjof Nansen's relief commissions in Russia and the Balkans. Because of these nonmilitary activities outside of the country, he was dismissed from his General Staff appointment, although he continued to draw reduced pay as a reserve officer.

The key to Quisling's subsequent political life can be traced to his shift from a friendly observer of Soviet experiments to that of harsh critic. He evolved quickly into an archconservative and a bitter anticommunist. On his return to Norway he served for a few months in 1932 as Minister of Defense in a government dominated by the Agrarians. More important, he found his new role as that of bitter anti-Labor party spokesman evolving to that of critic of all traditional parties. In association with other conservatives (from the extreme wing of the *borgerlige*) he

founded the National Union, a quasi-political party that he hoped would attract thousands of right-minded Norwegians. Hoidal devotes the better part of four chapters to the rather dismal experiences of Quisling and his movement during the depression years of the 1930s. Elections proved time and again that there was no popular support for the party within any segment of the population. The most distasteful blow was the advent to power of a largely Labor party government in 1935. Ever since returning to Norway with his Russian bride, Maria, Quisling had made clear his conviction that parties or persons associated in any way with international socialism were potential traitors to the accepted social order.

The real indictment of Quisling revolves about his relations with Nazi Germany, both before and after the occupation of 1940. Hoidal reinforces what is already well known about Quisling's flirtations with Alfred Rosenberg and Admiral Erich Raeder and his pro-Nazi statements and pretensions to power after the invasion of Norway on April 9, 1940. Several sometimes tedious chapters follow that describe Quisling's efforts to ingratiate himself with party and government officials in Germany in the hope of acquiring political power for himself and the Nasjonal Samling, which he headed. It adds up to a pathetic picture: a dreamer possessed of intelligence but little common sense; a would-be savior of Nordic civilization that was now threatened by the specter of communism embodied in the Red Army finally beginning its uninterrupted march to the West; and a presumptuous believer that he and his kind of Norway could play a salutary role in the power struggle then underway in Europe and the world. His actions, statements, and political pronouncements marked him clearly as a Nazi clone.

After Norway was liberated, Quisling defended himself stoutly in the courts, unable to conceive of having done anything treasonable in respect to the Norwegian constitution, the legitimate government, and the people. After a full trial, he was executed summarily. All of his plans, goals, and dreams died with him because, asserts Hoidal quite correctly, "they had never become part of the national consciousness" (p. 775).

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PETER BURG. *Die deutsche Trias in Idee und Wirklichkeit: Von alten Reich zum Deutschen Zollverein*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz; Abteilung Universalgeschichte, number 136.) Stuttgart: Franz Steiner. 1989. Pp. viii, 402. DM 98.

How many Germanies are there? The answer depends on when and what one counts. In 1985, for example, Karl Dietrich Erdmann argued that there were three German states (the Germanies east and

west and Austria) but only one German *Volk*. Some literary critics used to speak of four German literatures: East and West German, Austrian, and Swiss. Even today, when the situation seems to have been simplified by the disappearance of one Germany, the question of national identity remains.

Peter Burg's monograph examines the history of German multiplicity by studying the idea of a German Trias, the tripartite division of German-speaking Europe into the two major states, Prussia and Austria, plus a "third Germany" composed of the small and medium-sized states of the south and west. Burg traces the evolution of this notion from its origins amid the wreckage of the old Reich until the mid-1930s when the formation of a Prussian-led customs union opened a new phase in the relationship among the German states. The hero of Burg's story is Karl August Freiherr von Wangenheim, Württemberg's delegate to the German federal diet, who struggled to create a coalition of constitutional states as a balance to the authoritarian superpowers. But Burg can also find advocates of a Trias among other statesmen, publicists, and politicians. Moreover, he shows how the idea influenced the German states' deliberations over military policy, relations with the Vatican, and, finally, tariff policy.

In the end, of course, the Trias idea came to nothing. Even if the smaller German states had been able to unite, they would probably have been overshadowed by Prussia and Austria. And they were rarely able to unite for long: the middle-sized states constantly squabbled among themselves, and they could never establish a common ground with the small polities that still made up a majority within the German Confederation. Neither fear of Berlin and Vienna nor a shared sense of political values was strong enough to overcome the divisions and rivalries in the "third Germany."

The failure of the Trias poses some serious problems for Burg, whose account often has difficulty connecting the "ideas" with the "reality" of his subject. Occasionally the reader is lost in a tangle of tentative plans and vague proposals that seem rather far from the realm of political possibility. At times, what Burg presents as support for the Trias idea is difficult to distinguish from the narrowly defined self-interest of a southern state. Nevertheless, this study is a useful addition to a growing literature on alternatives to the Germany created by Bismarck in 1871. Like recent work on the Holy Roman Empire and the German Confederation, Burg's book illuminates aspects of the German past that were ignored by those who concentrated their attention on the fore-runners of the Kaiserreich. As such, it is of both historiographical interest and contemporary relevance.

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MICHEL KORINMAN. *Quand l'Allemagne pensait le monde: Grandeur et décadence d'une géopolitique*. Foreword by YVES LACOSTE. Paris: Fayard. 1990. Pp. xiv, 412. 170 fr.

The Germanies were a land of geographers long before the pseudoscience of *Geopolitik* developed. Author Michel Korinman, geographer and geopolitician, begins his capably detailed analysis with the accomplishments of early modern German cartography and the epoch-making studies of Alexander von Humboldt in the Americas. Political geography—not yet *Geopolitik*—arrived in the lifelong work of Friedrich Ratzel (1844–1904). He tried to reformulate descriptive geography toward a viable natural science by adding elements of anthropology and demography. From this combination Ratzel derived sets of categories, laws, and axioms of human behavior in motion within geographical space (*Raum*). Most other geographers of his era rejected Ratzel's heavily politicized work, as they achieved international prominence in cartography and in half a dozen top professional journals before 1914.

Non-German theorists such as Halford J. Mackinder of Great Britain and Rudolf Kjellén of Sweden had already given political geography further international scope before World War I broke out. Korinman details graphically how the events of that conflict focused German geographers' attention on *Mittleuropa*, Eastern Europe, Asia Minor, and the lost German colonial empire, efforts now ably buttressed by Austrian Germans who defended and explicated the German rationale of the Habsburg monarchy. When Berlin and Vienna both collapsed in defeat and revolution, the stage was set for a vigorous new turn in the already heavily politicized study and propagation of geography in the Germanic world.

Geopolitik had already been conceptualized by Kjellén in 1900. Korinman illuminates in significant detail how this variety of political geography became a "scientific" weapon in the German ideological arsenal to combat the alleged injustices of the Paris treaties. Here we meet Karl Haushofer (1869–1945), with his broad Pacific and Asiatic experiences and insights, and the new *Zeitschrift für Geopolitik*. Both now sought to give world-wide applicability and believability to the new ideological system to analyze, and even predict, international political developments. There was again sharp controversy, as it had occurred prior to the war, even among Germans, between the professional geographers and the new politicizing *Raumpolitik*.

It was, of course, the National Socialists who escalated *Geopolitik* into the ultimate conceptualizations and realization of their racial imperialism. Dissenting geographers fell silent soon after 1933. Korinman then shows interestingly how even the earlier geopolitical purists, such as both Karl Haushofer and his son Albrecht, ran afoul of the Nazis as the new German political masters bent and violated the "sci-

entific" precepts of this laboriously systematized knowledge to their fatal ends.

This excellent compilation and systematic analysis of a huge mass of political geographic literature ends with the fall of the Third Reich. Although the author finds the terms "geopolitics" and "geopolitical" still in current use, he avers that *Geopolitik* is dead. I would have wished for just a glance into the geographical studies of the late German Democratic Republic and some indication of trends in other German geography. Possibly Korinman's epitaph for *Geopolitik* is a bit premature.

The book is well buttressed with a bibliography and an immense sequence of endnotes that inspire confidence in its professional reliability. Still, there is a nagging note of Gallic insouciance: some two dozen colored and black-and-white maps are inadequately signaled in the text and are also left without sources, dates, or captions to link them to specific portions of the text—a lamentable oversight.

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ABRAHAM FRIESEN. *Thomas Muentzer, a Destroyer of the Godless: The Making of a Sixteenth-Century Religious Revolutionary*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xiii, 331. \$39.95.

TOM SCOTT. *Thomas Müntzer: Theology and Revolution in the German Reformation*. New York: St. Martin's. 1989. Pp. xix, 203. \$29.95.

Thomas Müntzer's life is the stuff of legends. A firebrand who traveled the road from mysticism to social revolution, Müntzer has been identified with the German Peasants' War of 1525 and as an inspirator for the Anabaptist movement. Martin Luther came to loathe him as Satan's agent, and long after Müntzer's demise, his name would still conjure up the specter of rebellion and religious radicalism. Rescued in the nineteenth century by Friedrich Engels and the German Social Democrats, Müntzer became the symbol of an alternative view of the Reformation espoused mainly but not exclusively by historians of the German Democratic Republic, who hosted a conference in 1989 shortly before the collapse of the Honecker regime to celebrate the quincentennial of Müntzer's birth. There is no doubt, however, that Müntzer's afterlife will long survive the GDR, as attested to by these two new biographies, which have separated the person of Müntzer from the myths that have grown out of the confessional, political, and historiographical interests of the past.

Tom Scott and Abraham Friesen approach the subject from very different perspectives. Scott, author of a study on Freiburg and a specialist on the German Peasants' War, is more interested in Müntzer the reformer and revolutionary. Although his biography does not neglect theology, it focuses on Müntzer, the

man of action. A succinct and reliable synthesis of recent advances in Müntzer scholarship, Scott's biography offers a vivid, convincing, and sympathetic portrait of Müntzer. Judicious in his arguments and evaluation of sources, Scott discusses in chronological order the various stages in Müntzer's life: from his obscure youth to first benefice in Brunswick to attendance at the Leipzig Disputation between Luther and Eck; then ministry in Zwickau; followed by a brief but important sojourn in Prague; thereafter, a long and fruitful ministry in Allstedt; exile and travel to south Germany; and, finally, participation in the Mühlhausen uprising, the battle of Frankenhausen, defeat, capture, torture, and execution. It is a story told with an eye to narrative details, without losing sight of the larger historical context. Scott imparts a sense of the complexity of Müntzer as husband and father, as a dedicated parish priest, as an earnest if bombastic reformer, and as a self-righteous and often cantankerous person. This biography succeeds not by proposing radically new views but in presenting a balanced, well-written, and complex portrait of a fascinating if not always likeable man. Like all good biographers, Scott exudes quiet sympathy for his subject without prejudicing the readers' ability to come to their own terms with Müntzer.

Friesen's book is very different. The author of a study on Marxist interpretations of the German Reformation, Friesen admits modestly in his introduction that "it [the book] hopes only to add one more building block to that edifice which will eventually constitute the biography of Thomas Müntzer" (p. 9). Deliberately excluded are any sustained analyses of the social, economic, and political contexts surrounding Müntzer's life. In fact, this is hardly a biography but more a collection of essays on the intellectual origins of Müntzer's thought. If one accepts this self-imposed limitation, there is much that Friesen has to offer.

Friesen's method consists of reading closely three authors frequently cited by Müntzer—Tauler, Eusebius, and Augustine—and identifying themes common to their theology. On Tauler, Friesen demonstrates in great detail Müntzer's indebtedness to the *Theologia Deutsch*, a thesis presented earlier by Hans-Jürgen Goertz, Steven Ozment, and Walter Elliger. On Eusebius, a more original section, Friesen offers a fresh reading of Müntzer's own feverish study of church history. This interest, first inspired by Müntzer's attendance during the Leipzig Disputation, was subsequently confirmed by his interpretation of the events of the early Reformation years. Finally, from Augustine, Müntzer derives his ecclesiology and chiliasm. Although Friesen concentrates on intertextual exegesis, he also offers insights into the political context for Müntzer's interpretations of his own times.

In general, Friesen presents a more detailed analysis of Müntzer's theology than does Scott, although the picture is somewhat fuzzy. Friesen never discusses

Müntzer's liturgical work from the Allstedt period, including the first German Mass, a work that suggests Müntzer's genuine popularity as a reform pastor. Another shortcoming is Friesen's selective and fragmentary inclusion of biological facts; we only meet Müntzer as flesh and blood halfway into the book and never learn, for example, that he married and fathered a child. The trouble with intellectual biographies, however well written, is that they tend to divorce texts from experience. Friesen is at his best when he can focus both on theological exegesis and Müntzer's experiences, but often Müntzer the individual is lost behind the thicket of textual exegesis. Toward the end of the book, however, Friesen breaks out of his self-imposed limitation to ask questions of the biographer. At the battle of Frankenhausen, where more than six thousand peasants were slaughtered, Müntzer fled the battlefield and was captured later in the city, hiding in bed pretending to be ill. For a boastful and self-righteous man, this was a humiliating end. In evaluating Müntzer's demise, Friesen's ambivalence and irony stand out in sharp contrast to Scott's sympathetic reading of Müntzer's final days.

In spite of their differences, both works demonstrate Müntzer's importance as an independent thinker and reformer who was never subservient or subordinate to Luther. Readers interested in theology may find Friesen's work disappointing compared to the thorough analysis of Walter Elliger (*Thomas Müntzer: Leben und Werk* [1975]). Readers who want a succinct and reliable biography of Müntzer should pick Scott, one of the most accessible works among existing German and English biographies of the radical reformer. It is a pity that both books have reproduced inaccurate portraits of Müntzer—Scott's from a contemporary East German painting and Friesen's from a seventeenth-century Dutch engraving. For the only accurate sixteenth-century portrait, one needs to consult Hans-Jürgen Goertz's excellent biography, *Thomas Müntzer: Mystiker, Apokalyptiker, Revolutionär* (1989).

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KATHARINE ANNE LERMAN. *The Chancellor as Courtier: Bernhard von Bülow and the Governance of Germany, 1900–1909*. New York: Cambridge University Press. 1990. Pp. xiii, 350. \$49.50.

Until the recent publication of Otto Pflanze's monumental *Bismarck and the Development of Germany* none of imperial Germany's eight chancellors had been the subject of thorough, professional biographies. There were useful studies of their careers but not of their lives. No one stands more in need of biographical attention than Bernhard von Bülow, whose personality dominated the government in Berlin from 1897, when he was appointed state secretary of the Foreign Office, until his dismissal from the chancellor's palace

almost twelve years later. Katharine Anne Lerman's welcome volume does not aspire to be a biography; like the admirable monographs of Peter-Christian Witt and Peter Winzen, it aims modestly to probe only certain elements of Bülow's ultimately unsuccessful career. The temptation therefore arises in a reviewer to be mindful both of what is unsaid as well as of the deft discussion in this book of many important features of Bülow's regime.

We are treated to an authoritative dissection of imperial politics from 1900, when Bülow assumed the chancellorship, to his fall in 1909, a narrative that is developed with great density and with an avalanche of footnotes, some of them perhaps gratuitous. Bülow took office possessed of many advantages, the most valuable being his long-established friendship with the antic Kaiser, Wilhelm II. Among the chancellor's gifts was a polished manner that was usually beguiling but sometimes suspicious. His diplomatic colleagues for good reason referred to Bülow as "the eel"; his own brother deplored his alarming lack of character; and Lerman convincingly adds to this compendium of faults by showing that Bülow, if replete with ambition, was virtually devoid of ideas. To the chancellor, politics was little more than a course of strategic improvisation designed to avoid conflict and insure parliamentary majorities. For the actual work of government he had only a trivial concern, and his energy was devoted instead to ingratiating himself with the Kaiser. Bülow was the perfect courtier for Wilhelm II, possessing depths of sycophancy that no other chancellor even approximated. But this was not enough. Bülow was expected to share the monarch's prejudices, to embrace (at least tentatively) England, and to scorn Catholics as aliens and Social Democrats as traitors. The chancellor unfortunately preferred Russia to England, a troubling inclination that Lerman might profitably have examined. Although conventionally Protestant, Bülow was attached by sentiment as well as by political calculation to the Catholic Center party, and his stentorian advocacy of conservatism stopped considerably short of Wilhelm II's vitriolic hatred of both Catholics and Socialists. Lerman makes very clear how the Bülow regime, conceptually vacant and therefore incapable of sustaining a parliamentary majority, was fragile from the start and later seriously undermined with both the Kaiser and the Reichstag by a number of crises in 1905–06. The *Daily Telegraph* imbroglio late in 1908 was merely the *coup de grâce*.

There is much to be learned in this valuable book about domestic affairs and the nature of government in the first decade of this century. Lerman's command of the sources is impressive, her arguments generally persuasive, and she is judicious in her conclusions. Foreign affairs, for which Bülow had considerably more interest than domestic contentions, receive little attention except where they impinge on the government's troubled dealings with the Reichstag or the Kaiser. Bülow was not only a bureau-

crat and parliamentary skirmisher but also a diplomat, and an examination of this side of his career and its integration with his unimpressive domestic performance remains to be written. In Berlin politics, the role of the court and its fixtures, many of them hostile or at least very wary of Bülow, could bear additional scrutiny. This is especially true of the military nexus, to which Bülow had many ties and to which he proclaimed a profound allegiance. The soldiers who thronged the Hohenzollern court under the last Kaiser strangely make almost no appearance here, even though their opinions on both domestic and diplomatic affairs, and the connection between the two, had enormous influence on the Kaiser. Lerman's tightly focused examination of the Bülow regime amply confirms how paramount a place in German politics was held by Kaiser Wilhelm II, a man quite as egomaniacal, as wantonly theatrical, as insouciant about his responsibilities, and as devoid of serious ideas as was his courtier-chancellor Bülow, the man Wilhelm had once vainly proclaimed would be a second Bismarck.

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PETER FRITZSCHE. *Rehearsals for Fascism: Populism and Political Mobilization in Weimar Germany*. New York: Oxford University Press. 1990. Pp. x, 301. \$39.95.

Peter Fritzsche's interesting and significant book fills several important lacunae in the historical literature on the Weimar Republic. In the first place, it is a regional history—in this case, of a geographical area in central Germany known as Lower Saxony—that seeks to bridge the gap between local and national history and thus enrich our understanding of Germany's national political culture with the intimate texture that only local history can provide. Second, it is a study of the political behavior of that social stratum that was to provide the Nazi movement with so much of its popular support, yet about which we continue to know relatively little, namely, the urban middle classes in the small- and middle-sized cities that dotted the landscape of the German heartland. Third, it addresses an aspect of Weimar political culture that has thus far escaped serious historical attention, that is, the forms and modalities of bourgeois political mobilization in the decade before the emergence of Nazism as a mass political movement. The success with which Fritzsche integrates these three dimensions of historical analysis into his assessment of bourgeois political behavior in Lower Saxony between 1918 and 1930 represents one of the enduring accomplishments of the book.

Fritzsche's central thesis is that with the decline of Germany's traditional bourgeois parties in the second half of the 1920s, a distinctly populist impulse emerged that was directed not merely against established political elites but also against the role that

organized economic interests had come to play in the Weimar party system. This impulse drew much of its inspiration from the sense of national unity that had surfaced with the outbreak of World War I and represented an attempt to translate the "ideas of August 1914" into social and political reality. Reinforced by the mythos of the front experience as articulated by Ernst Jünger and other neoconservative theorists, this impulse assumed concrete form in the second half of the 1920s in the crusade of the Stahlhelm and other patriotic associations for a reform and rejuvenation of German political life that would free it from the domination of outside economic interests. More important, the spirit of civic activism that found expression not only in the Stahlhelm's struggle to regain control of the city square but also in the Landvolk movement's protest against higher taxes and land rents anticipated rituals of political mobilization that the Nazis were to adopt with even greater effect following the outbreak of the world economic crisis. Thus, a direct line of continuity can be traced from the populist politics of the late 1920s to Nazism's electoral breakthrough at the beginning of the following decade.

Fritzsche's book is based on exhaustive research in a wide range of national, regional, and local archives. The author is particularly resourceful in his use of local newspapers to recapture the texture of local political life. For all of its strengths, however, Fritzsche's work is weakened by a serious, although hardly fatal, flaw. In his attempt to draw conclusions for an understanding of Germany's national political culture from his research on Lower Saxony, Fritzsche fails to appreciate the limits of his evidence and creates the misleading impression that what is true for Lower Saxony is true for the rest of Germany. Fritzsche's failure to comment on the extent to which the patterns of political development that emerged in Lower Saxony replicated themselves in Württemberg, Bavaria, or other parts of the country, therefore, raises unanswered questions about the validity of his argument for Germany as a whole. As a result, one must be careful in extrapolating conclusions for the rest of the Reich from what Fritzsche says about Lower Saxony. None of this, however, vitiates the overall usefulness and value of Fritzsche's work as a contribution to the historical understanding of Weimar political culture.

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SYLVIA FERRETTI. *Cassirer, Panofsky, and Warburg: Symbol, Art, and History*. Translated by RICHARD PIERCE. New Haven: Yale University Press. 1989. Pp. xix, 282. \$30.00.

During the 1920s the Warburg Institute in Hamburg was one of the great modern centers of art history, and the three men considered in this book—Aby

Warburg (1866–1929), Ernst Cassirer (1874–1945), and Erwin Panofsky (1892–1968)—were the most distinguished figures associated with the institute before it moved to London in the 1930s. Sylvia Ferretti's interests lie not so much in the historical culture that made these three minds possible as in the theoretical issues about art, symbol, and human consciousness that these authors raise. Ferretti's mode of addressing these themes does not make its way easily into the kind of English prose most historians are comfortable with, but the translator sometimes makes matters unnecessarily difficult, as when he changes Ferretti's title despite the fact that it represents the order of her argument: *Il demone della memoria: Simbolo e tempo storico in Warburg, Cassirer, Panofsky* (1984).

In the three chapters that constitute the main body of the book, Ferretti deals in turn with the ideas of Warburg, Cassirer, and Panofsky, concentrating primarily on their work from the 1920s and attempting "to demonstrate the differences and in part the obvious contradictions" among them (p. xv). Ferretti emphasizes the interests of these men in the nature of artistic imagination and in the way symbolic meanings unfold historically, but she sometimes loses track of her themes as she gets drawn into specific texts and influences. She is sharply critical of Cassirer's view of symbols and historical evolution; her sympathies are more with Warburg and Panofsky and with the pessimism of Arthur Schopenhauer and Friedrich Nietzsche. Indeed, her admiration for Warburg as one of the great figures of the twentieth century is evidently linked to the role of suffering in his own life, which she mentions only obliquely despite its connection to images associated with melancholy and the demon of memory. In this sense the deeper concerns of the book are not so much historical as psychological, although this is not made explicit except for her references to Warburg's psychic suffering and his hospitalization from 1918 to 1923, the "psychotic years" that Ernst Gombrich characterizes briefly in *Aby Warburg: An Intellectual Biography* (1970, p. 215).

Three highpoints of Western civilization interact in this book: Greek classicism, the Renaissance, and Imperial German culture—itself very much a culture of memory as the young Nietzsche pointed out. Warburg and Cassirer shared a passion for ancient Greece as the origin of civilization, but Warburg looked for crises and transitions whereas Cassirer emphasized evolution and reconciliation. The Renaissance was crucial for Warburg as an age of transition in which artists drew on Greek archetypes to help in the perennial human task of overcoming fear and irrationality. A good portion of the Warburg Institute's significance arose from the importance it attributed to astrology, pagan demonology, superstition, and in general to sources that are least accessible to modern consciousness (p. 222). Ferretti's quarrel with Cassirer stems less from his specific views on Immanuel Kant or symbolism than from what she regards as

a somewhat one-dimensional view of the victory of modern scientific consciousness that allows no place for other permanent dimensions of human consciousness and experience. In this respect Ferretti's book links up with the work of Frances Yates on astrology and memory.

Warburg was interested in the power of images to help us master painful emotions, and both he and Panofsky were particularly fascinated with Albrecht Dürer's engraving of 1514, "Melancholia I." This engraving epitomized the concerns of the Warburg Institute with the capacity of art to master the primitive demon of memory, and Dürer's engraving, uniting the suffering of depression and the star of contemplation, emerges as the recurring motif of Ferretti's book. Here Dürer is associated with Martin Luther's struggle against demonic superstition, the struggle to spiritualize the pagan superstitions of the Renaissance, to spiritualize astrology and humanize the demon.

Ferretti's discourse is evocative at times, especially when she is writing about Italy or the Renaissance, but her book would have benefited from a more coherent sense of the shared intellectual context of these three minds. The creation of the Warburg Library and the preoccupation of these men with the history of art and human consciousness imply another culture that has gained a quality of remoteness and historical distance that was not possible a generation ago. Ferretti emphasizes the importance of life and will in nineteenth-century German accounts of Renaissance creativity, but what is missing is the intellectual world of these German writers—and the life and will that shaped their theories of Renaissance art.

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W. E. MOSSE. *The German-Jewish Economic Elite: A Socio-cultural Profile*. New York: Clarendon Press of Oxford University Press. 1989. Pp. 369. \$65.00.

This volume can best be appreciated as a sequel to Werner E. Mosse's *Jews in the German-Jewish Economy: The German-Jewish Economic Elite 1820–1935* (1987), which was a systematic study of the role of entrepreneurs of Jewish origin in the German economy based on a well-defined sample of extremely wealthy German Jews. The new volume deals with the mentality and social position of this same elite. The earlier volume gave a good deal of hard data, most of it indicating that the economic role of Jews in the German economy was greater than generally assumed. Because surviving sources on the mentality, cultural activities, and friendship and marriage networks of the German-Jewish elite are fragmentary, this volume is much more impressionistic than the first. Relying, perforce, on conventional literary

sources such as memoirs, correspondence, and genealogical information, the new volume reads more like the "old social history" than like the new. Mosse constructs Weberian ideal types that he then illustrates with many concrete examples based on primary sources.

This volume leaves a different impression from the earlier book in its conclusions as well as its methodology. Although Mosse claims to lean cautiously to a more optimistic view, the picture given in this volume tends to be a somber one. Whereas the first volume showed a powerful and economically successful elite, this volume is filled with cases of blocked integration, social rejection, unsuccessful social climbing, and anti-Semitic unpleasantness—obstacles, moreover, that increased over time. Although there were some friendships between wealthy Jewish entrepreneurs (including the famous *Kaiserjuden*) and prominent Gentiles, these tended to be more instrumental than deeply personal. Even baptized Jews found it difficult to become completely integrated into the Gentile elite. Mosse finds clear differences between the Jewish elite and German non-Jewish entrepreneurs in their economic and social principles (Jews were more committed to liberalism and free trade), cultural interests, and family patterns.

A wide spectrum of attitudes toward Jewish identity among the elite, ranging from strict Orthodoxy to complete assimilation, is described. The many elite Jews who converted to Christianity and the difficulties and successes of their families in integrating are chronicled in considerable detail. Although the story of baptized Jews is certainly important for understanding barriers to persons of Jewish origin in Germany, one wonders whether Mosse does not go too far in simply including them in the Jewish elite. Perhaps some part of the study should have treated these baptized Jews as a separate category measuring their relative numbers and wealth and differences between their sociocultural experience and those of Jews by religion.

Mosse defends the reputation of the Jewish elite against Gershom Scholem's depiction of them as assimilated parvenus trying to push their way into a Gentile society that did not want them. He also believes that the picture of the elite has suffered from too much emphasis on such atypical elite members as Gerson Bleichröder and Walther Rathenau.

One gets the impression that the political activities, cultural interests, and social ambitions of the wealthy elite described in this book were common to many German Jews. Some attempt at exploring the question of whether or not the elite was distinctive compared to the rest of German Jewry might have added to the value of the volume.

Mosse's study is generally well balanced, although he is sometimes a bit too anxious to see the positive side of the elite. Overall, despite a somewhat old fashioned feel, this is a well-informed, intelligent, and entertaining volume that will help to flesh out our

views about the integration of Jews into German society.

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PHILIPPE BURRIN. *Hitler et les Juifs: Genèse d'un génocide.* (XX^e siècle.) Paris: Seuil. 1989. Pp. 200.

In the debate over the genesis of the "Final Solution," some historians known as "intentionalists" have argued that Adolf Hitler opted to realize his premeditated plans to murder the European Jews at the "opportune" moment of Barbarossa. In contrast, "functionalists" have played down Hitler's role and argued that the volatile Nazi system began improvising mass murder when the Russian campaign and plans for expelling the Jews broke down in the fall of 1941. Historians for a "middle" position (myself included) have argued that Hitler played a key role in the decision-making process but not out of premeditation. Frustrated by the failure of previous solutions (emigration and expulsion), he opted for the Final Solution in the "euphoria of victory" of midsummer 1941.

Philippe Burrin now contributes a second middle position to the debate (accepting Hitler's premeditation but arguing for a rather late date on the Final Solution decision) that he dubs "conditional intentionalism." In Burrin's view, Hitler in the 1920s saw a solution to the Jewish question in terms of expulsion, resettlement, and hostage-holding but not extermination. By the mid-1930s, however, Hitler envisaged an alternative. On September 25, 1935, Hitler intimated to Walter Gross concerning the Jews that in case of a war "on all fronts," he was ready for "all the consequences" (p. 48). Thereafter, according to Burrin, Hitler followed a consistent policy. As long as he was successful, the Jews faced expulsion; once his expansionary goals were checked, the Jews faced extermination. That was the meaning of his threat in the Reichstag in January 1939; if the Jews provoked another world war, it would result in the destruction of the Jews in Europe. In Burrin's view, just such a check to Hitler's plans occurred in August and September 1941. Under these new "conditions," Hitler opted for the murderous alternative. Burrin's key documentary discovery is a letter from Reinhard Heydrich explaining the decision to allow the bombing of Parisian synagogues on October 2, 1941. According to Heydrich, the decision was taken "only at the moment when from the highest authority the Jews were emphatically branded as the incendiaries of Europe who must ultimately disappear" (p. 141).

There is much to admire in Burrin's diligent research, painstaking attention to detail, and careful argument. Any historian interested in this debate must consult and take his book seriously. But, in the end, I remain respectfully unconvinced by Burrin's

conclusions. Several aspects are particularly problematic. First, he argues that the systematic mass murder of all Russian Jews was not clearly being implemented until mid-August and then illogically dates a decision by Hitler to that point, as if the decision followed rather than preceded implementation. This sleight of hand is necessary to maintain any plausibility in his theory that Hitler's decision to murder the Russian Jews was also determined by the frustration of defeat rather than euphoria of victory. More logical would be to date Hitler's decision to his mid-July call for intensified pacification, after which Heinrich Himmler suddenly reinforced the number of killers in the east by two SS brigades and eleven police battalions (thus adding nearly sixteen thousand troops to the mere three thousand men of the over-extended *Einsatzgruppen*) and also approved a vast increase in the number of native auxiliaries to be recruited. Second, Burrin argues even more emphatically for the existence of an unmitigated mood of growing frustration and defeat in September 1941 that led Hitler to extend the mass murder to the rest of Europe. In so doing, he employs the intentionalist methodology of selective citation, dismissing all contrary statements by Hitler as merely putting on a good face. The renewed euphoria surrounding the fall of Kiev and the victory at Vyazma and Bryansk are ignored, as are military and economic decisions that do not reflect the despairing state of mind Burrin attributes to Hitler. For instance, as late as October 23, 1941, plans to demobilize the equivalent of forty-nine divisions were reaffirmed by the armed forces high command—hardly an indication that Hitler was already resigned to a prolonged war on all fronts. Finally, Burrin sees Hitler's "decisions" for the Final Solution embodied in precise orders almost instantly implemented, whereas I see them as the aggregate of a series of signals gradually understood, unevenly disseminated, and slowly implemented.

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H. W. KOCH. *In the Name of the Volk: Political Justice in Hitler's Germany.* New York: St. Martin's. 1989. Pp. xv, 325. \$35.00.

In April 1934 the National Socialist regime established a new court, the Volksgericht, to take over jurisdiction in crimes of treason. Like the special courts set up a year before to try offenses under the Reichstag fire decree, the Volksgericht, or People's Court, stood alongside the existing courts (in this case the Reichsgericht) and expressed the regime's dissatisfaction with the standards and procedures embodied in the judicial system inherited from the Weimar Republic. Yet the Volksgericht was neither unconstitutional nor unprecedented but in structure and function resembled the Staatsgerichtshof established by the republic in 1922 as part of its package of

belated measures to protect the constitution. Not until World War II did it really begin to function as the brutal parody of justice with which it has become synonymous. In this sense, the Volksgericht was intermediate between the normative legal system inherited by the Nazi regime and the prerogative system increasingly embodied in the Gestapo and SS.

H. W. Koch's new book offers an illuminating account of the court's history and Roland Freisler's role in it, culminating in the infamous trial of the conspirators of July 1944. It supplements Walter Wagner's *Der Volksgerichtshof im national-sozialistischen Staat* (1974) and William Sweet's brief article in the *Journal of Modern History* in the same year (which, curiously, Koch does not cite). Koch has already published widely on the history of National Socialism and in his recent constitutional history of Germany argued forcefully that the Nazi takeover of power was procedurally and substantively legal. It is therefore not surprising that he should now turn to the problem of Nazi justice itself. Koch largely follows existing conventions, emphasizing, for example, the Weimar precedents for the Volksgericht and the conservatism of its verdicts and sentencing before the war. Similarly familiar is his depiction of the German judiciary as having been thrust after 1918 into an unwelcome political role, which in a sense softened them up for the Nazi regime after 1933, although not as much as their new masters wanted. His central argument, in fact, is that the slightest departure of law and the judicial system from the principles of strictest depoliticization is fraught with dangers (although whether the judicial system in imperial Germany was apolitical, as Koch argues—indeed, whether it ever can be—may be doubted). The extended discussion of Freisler's legal views is valuable, and Koch is right to argue that, on top of his ideological commitment to the Nazification of German law, Freisler had the more straightforward ambition of supplanting the Reichsgericht as Germany's supreme court—a typical combination of personal and political priorities for the younger generation of careerist lawyers and officials in Nazi Germany. Koch is also on firm ground in arguing that Freisler's concepts of loyalty (*Treue*) and treason reflected a broader Nazi fear of a repetition of November 1918—of a popular loss of faith in the state and its policies that might begin with falling morale but then escalate into a more active withdrawal of support.

Given Koch's own complaint against Wagner's earlier study for its analytical shortcomings, it is something of a loss that he himself does not address the wider context of Nazi legal and political theory examined, for example, by Rudolf Echterhölter (public law), Bernd Rüthers (private law), Michael Stolleis (the concept of the common good), and Dietrich Kirschenmann (law and statute). It is true that the scope of legal debate was narrowly circumscribed under National Socialism, but these and other authors have shown that both the contemporary debates

and the theories themselves can illuminate the ideological origins and self-image of the Nazi state and law. Koch's judgments are also somewhat insecure in places. It is not true that in Weimar "the judiciary was just as harsh [with right-wing offenders] as it had been with Communists" (p. 11). On the contrary, by 1921, in the thirteen cases of murder in which leftists were brought to trial, 8 death sentences and 176 years of imprisonment were imposed; by contrast, the 314 cases of murder in which rightists were tried attracted one life sentence and 31 years in other sentences (see Volker Berghahn, *Modern Germany*, p. 76). Here, and in some other instances, Koch does not give us enough of the evidence on which his conclusions are based. The text's chronological leaps and switches, digressions, and unassimilated case studies and documentation also make it hard to follow in places. Text and footnotes would have benefited from more meticulous editing.

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HERBERT F. ZIEGLER. *Nazi Germany's New Aristocracy: The SS Leadership, 1925–1939*. Princeton: Princeton University Press. 1989. Pp. xx, 181. \$29.95.

Herbert F. Ziegler has used a representative sample of SS officers culled from the Berlin Document Center to determine the sociological make-up of the blackshirted leaders, and, insofar as it is successful, his study supersedes earlier ones, for instance those by Gunnar Boehnert and Bernd Wegner. Ziegler has used quantitative techniques to do this, and hence he implicitly reasserts the value of a historiographical tool that some historians have already deemed buried. To the best of my knowledge, the author's use of that tool is sophisticated enough to inspire confidence in his figures; he also supplements an impressive collection of archival materials with most of the known critical literature in the immediate area of Nazi social history and, in particular, the SS.

Ziegler's main findings are that the SS (Schutzstaffel) represented an attractive vehicle of social mobility for wide sections of the German male population, by way of quick officer career advancement, and hence could be looked on as a viable alternative to the more traditional and cumbersome Reichswehr or Wehrmacht. With certain variations over time and depending on the type of SS formation analyzed, Ziegler can prove that workers, the (lower) middle class, and the social elite were all welcomed by and found a home in the black formations. This is a meaningful step beyond earlier works, notably Heinz Hoehne's, who in the 1960s emphasized the upper-crust character of Heinrich Himmler's officer recruits. Ziegler demonstrates that Himmler's quasi-egalitarian intent, to allow each man to prove himself to the best of his abilities, actually met with success in practice and thus made good some of the party's

claims of social opportunity. A related finding is that, in terms of its pronounced youthfulness, the SS contrasted markedly with the increasingly senescent pedestrian echelons of the Nazi party itself.

The author has wisely adopted, as his fundament for comparison, matrixes tried out by other social historians of the Third Reich, myself included, who based their classifications on models by Max Weber and Theodor Geiger. This now makes Ziegler's various comparisons of his specific Nazi social group with others in the Nazi realm convincing and thus provides a working base for future analyses of this kind, for instance of the German Labor Front (DAF) or the Labor Service (RAD) hierarchies. But, on the minus side, three points of criticism must be raised. First, the author puts too much credence in certain obscure secondary works, such as a two-page article by Yoash Meisler or the confusing book by John M. Steiner, *Power Politics and Social Change in National Socialist Germany* (1979). Second, the work lacks a binding conclusion or, rather, has what could pass as one tucked into its overly long introduction, thus appearing to prejudge many important issues at the outset. And, third, instead of supplying numeric tables containing precise figures for further processing, Ziegler has resorted to three-dimensional diagrams that seem to have jumped right out of the pages of the official Nazi *Parteistatistik* yet do little more than cause the reader to marvel at the superb craftsmanship of the distinguished publisher's draftsmen. Altogether, this modest-sized volume is testimony to the fact that the SS continues to fascinate researchers of the Third Reich; Himmler's order by now is the most frequently scrutinized social unit of Hitler's dictatorship. One might wish that progress would be made soon in other hitherto neglected sectors of Hitler's realm, for instance the universities, the arts, or, indeed, popular culture.

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GEORGE C. BROWDER. *Foundations of the Nazi Police State: The Formation of Sipo and SD*. Lexington: University Press of Kentucky. 1990. Pp. xii, 346. \$35.00.

On June 17, 1936, Adolf Hitler issued a decree that invested the Reich leader of the SS, Heinrich Himmler, with the newly created office of chief of German police. This meant that Himmler was now in the position to realize his aim of creating a *Staatsschutzkorps*, a state protection corps that was comprised of all regular uniformed police (*Orpo*), the security police (*Sipo*), a fusion of criminal (*Kripo*) and political police (*Gestapo*), and the SS security force, the *SD*. Behind this alphabet soup of office designations loomed the ominous reality that German police forces, which until this time had been under the jurisdiction of individual states, were now centralized and that authority over the police was transferred

from state or administrative authority to party and SS control. The amalgamation of SS and police, according to George C. Browder, laid the firm foundation of the Nazi police state, which, up to this point, had evolved in a piecemeal fashion amid complex power struggles and, most notably, without Hitler's direct involvement or support.

Conceding that Hitler's approval was ultimately necessary for Himmler's success, Browder also makes clear from the outset that Himmler's final triumph was not a foregone conclusion. Because there were no established guidelines for what to do with Germany's police forces in the wake of the Nazi victory in 1933, and because Hitler typically failed to provide any definite directions, the stage was set for political infighting. Browder successfully demonstrates that the Darwinian rivalries and administrative chaos surrounding the growth of the police state emanated from Hitler's inability to create systematically an authoritarian government rather than from a consciously conceived strategy of "divide and rule," as has often been assumed. When the smoke of battle cleared, Himmler had outplayed and outlasted each of his opponents, offering Hitler the most suitable security instrument for the future.

In describing and analyzing the origins of the Nazi police state, Browder puts himself squarely into the thicket of a continuing historiographical debate concerning Hitler's role and function within the Nazi system of rule. In contrast to the so-called intentionalists, who have emphasized the centrality of Hitler's ideas and personality in shaping and determining the fate of the Third Reich, Browder, in the fashion of those labeled "functionalists," focuses on the structure and the functional nature of decision making. It is at this point, however, that Browder gets into trouble. Not content to limit himself to a "polycentrist-functional" interpretation, which is supported by a preponderance of his own evidence, he also seeks a "transcendent synthesis" of "intention" and "structure" so as not to neglect the importance of Hitler and his ideology.

Although his effort at synthesis is admirable, Browder weakens his predominant functional interpretation without really establishing intentionalist factors. For example, Browder stresses Himmler's ability to proffer a vaguely defined ideological mission, which rationalized his pursuit of power as a fulfillment of Nazi ideals in contrast to his powerful opponents who appeared to seize the instruments of police power merely for their own sakes. If Himmler's grand schemes were so crucial for his success, why does Browder not scrutinize these intentions further, and why does he disregard the works of Yoash Meisler and Bernd Wegner? Both authors have established that Himmler viewed the SS at once as the protector of the Third Reich and as the architect of Hitler's New Order. Such a mission virtually dictated that the Black Order embrace and fuse those forces of the state that were viewed as indispensable to its designs,

particularly the police. And, although Himmler is portrayed throughout the work as building the foundations of a police state without Hitler's guidance or backing, a lonely remark also informs us that Hitler was clearly central to everything that happened (p. 247). Perhaps Browder's promised sequel to this work will clarify some of these issues. Then again the proverbial beast, perhaps by its very nature, may always frustrate attempts to synthesize existing disparate historiographical positions.

All in all, this very detailed study, supported by a wealth of archival sources, makes available new insights into the process of the nonsecretive, gradual, and haphazard evolution of the Nazi police state. As such, it provides a welcome addition to the literature on National Socialist Germany.

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MARK WALKER. *German National Socialism and the Quest for Nuclear Power, 1939–1949*. New York: Cambridge University Press. 1989. Pp. x, 290. \$29.95.

Why did the Germans lose the race for an atomic bomb during World War II? This is a common question asked by undergraduates and general readers alike, despite the many books written on the subject. A persistent tendency exists to think that the West, with its open society, greater resources, and better leadership, won the race. According to Mark Walker, there is much truth to this view. In a cool, dispassionate study of the relationship between the German scientists and their modern state, he argues that the quest for nuclear power in the last six years of the Nazi era can only be understood within the context of the power configuration within the whole society, including its civic and moral dimensions.

Starting with a brief review of the position of scientists in Imperial and Weimar Germany, Walker argues that most scientists sought to be apolitical but sympathized with the Nazis when they came to power. When the war started, they carried out their assigned military research conscientiously and enthusiastically as good Germans. Nuclear fission research was well supported and quite successful. Although scientists thought heavy water appeared more promising as a moderator than pure carbon for the uranium machine, their research was at the same level as that in the West. By early 1942, however, the army, industry, the Nazi government, and the academic and military scientists believed that the applied nuclear fission program was irrelevant to winning the war. Precisely when the West assigned its program the highest priority, the Germans downgraded theirs, believing that other projects showed more potential of developing before the war was over. The decision was based on their limited economic resources, competing projects, and the expectations of a short war. After

that, there was no race. The German program plugged along, plagued by all of the problems of war, but it never would match the West, as the Germans found out after the war.

Did the German scientists delay or hinder the production of the weapon for Hitler because of their moral reservations? Walker finds little evidence to support this view that was spread after the war. Most scientists, including Werner Heisenberg, were as caught up in German cultural imperialism as were many other Germans who had misgivings about the Nazis. Indeed, judging from the limited number of seventy-one scientists involved in the program, as a group they were more enthusiastic than average. This is not to say they were pro-Nazi. They were not. Most were honorable men in a difficult situation. Sometimes they opposed Nazism, other times they supported it, acquiesced, or simply were not aware of its consequences. The author has put to rest the many "myths" about the German scientists' opposition to Hitler, including the most pernicious that they were apolitical and, like the generals, that things might have been different had Hitler listened to them.

This is an excellent book: intensely researched, well written, and balanced in its judgments. Its focus on the German scientists is novel, and its discussion of the position of science in a modern state is well worth pondering. Walker's thesis that German scientists retrospectively had to convince themselves and the world that they had acted in a professionally acceptable manner is similar to the pleas of other professional groups after the war. What was different, as the author proves, is that the cultural, economic, political, and scientific environment in Nazi Germany was far different from the West, and that this factor ultimately determined the fate of the nuclear power program.

EDWARD L. HOMZE
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REINER POMMERIN. *Von Berlin nach Bonn: Die Alliierten, die Deutschen und die Hauptstadtfrage nach 1945*. Cologne: Böhlau. 1989. Pp. viii, 262.

Completed prior to the extraordinary changes of 1989–90, Reiner Pommerin's book will be read in the light of events he could not foresee. He discusses the selection of Bonn as the meeting place for the Parliamentary Council, its designation as the provisional capital, and then its gradual acceptance as the permanent capital of the Federal Republic of Germany. This meant not only the victory of Bonn over its main competitor, Frankfurt, but also the abandonment of Berlin by West Germany as the capital, although Berlin did become the capital of the German Democratic Republic. Only in 1975 was an agreement reached among the city, the state of North Rhine-

Westphalia, and the federal government that made possible a long-range urban plan for the capital. Democratic, prosperous West Germany had become firmly identified with its modest capital city.

The usual story is that Bonn was chosen because it was close to Konrad Adenauer's home, but Pommerin argues that Adenauer was not the one to initiate Bonn's candidacy. Berlin was rejected because it represented Prussian militarism and excessive centralism and because it was subject to pressure from the Soviets. The British preferred Bonn since it was one of the few cities in their zone that was not too badly destroyed. Adenauer advocated Bonn not just because of personal convenience; he saw that placing the capital on the left bank of the Rhine precluded any French move to split off the Rhineland into a separate German state. Moreover, the British were able to pressure the Belgians, the occupying force in Bonn, to evacuate the city and turn it into an occupation-free enclave, a significant gesture toward the new government.

Other cities, including Stuttgart and Frankfurt, were suggested as sites for the Parliamentary Council and as the seat of the first Bundestag, but Frankfurt, which had the support of the Social Democratic party, was Bonn's only serious rival. Frankfurt, however, was in the American zone. The Americans not only took up much of the usable space but also were unwilling to evacuate their forces completely. Once the first Bundestag met in Bonn, other agencies "voted with their feet" by relocating there, and the British moved their headquarters near Bonn, all investments that no one wanted to waste through another move.

In the 1950s and 1960s, in order to signal their continued belief in eventual unification and to support beleaguered Berlin, West German politicians reasserted Bonn's provisional status and maintained at least a symbolic governmental presence in Berlin, for example, as with the second residence of the president. With the flowering of the *Ostpolitik* and the tacit recognition of the GDR in the early 1970s, however, Bonn's status seemed secure.

This topic is now open to question; the path that Pommerin has so carefully traced has suddenly reversed itself. The unification treaty of August 1990 declared Berlin to be the capital of a united Germany, with the seat of government to be determined later. Now it has been decided that most government institutions will also move to Berlin. Pommerin's interesting and succinct account is based on thorough research in German archives, the archives of the occupying powers, and the voluminous published materials from the early postwar years. It will be unfortunate if this solid contribution to postwar history gets lost in the excitement of reunification.

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BARBARA MARSHALL. *The Origins of Post-War German Politics*. New York: Croom Helm. 1988. Pp. 221. \$57.50.

Contrary to its sweeping title, this book is a historical case study of politics in British-occupied Hanover after World War II. Barbara Marshall's study consists of three major parts. She analyzes the evolution of British occupational policies and practices in Germany, traces British-German political interactions in Hanover, and evaluates the response of Germans, and especially the Left, to the new political situation. Her brief conclusion is a review of the effects of the British occupation. It is with an eye on later, rather conservative, political developments in West Germany that she poses two central questions. First, why did the British Labour government fail to "export" a vigorous socialism to its zone of occupation and to Hanover? And second, why did the German Left, and particularly the politically predominant Social Democratic party (SPD) of Hanover, institute so few local improvements and generate so little broad enthusiasm?

To scholars, Marshall's questions (and some of her conclusions) will not be particularly new. After all, the lack of vitality of the postwar Left in Europe and Germany has been examined and debated for some time. Nevertheless, Marshall's study merits consideration. It deepens our insight into the local setting. She furnishes additional information to test larger questions, and she shows the utility of a local focus for an understanding of political developments in postwar Germany. For, as she emphasizes early on, politics and many other activities in fragmented and disorganized Germany between 1945 and 1948 were necessarily local.

Marshall's choice of Hanover is quite appropriate. Because of its prominence as a regional capital, Hanover became a center of the British zone of occupation. The city was heavily damaged during the war, but it remained an industrial hub with a large working-class population. In Hanover, the policies of the new Labour government in London, the practices of the British occupational forces, and the conduct of German politics converged. It was here that ambitious plans for a nation-wide unified general trade union organization were forged. And it was in Hanover that the SPD of western Germany was re-founded and enlarged under the energetic leadership of Kurt Schumacher.

Marshall addresses all of these issues. Her conclusions are often critical. The British failed to export socialism for many reasons: they had been greatly weakened by war; they were burdened with the administration of a terribly devastated and impoverished zone; they tended to distrust all Germans, including those on the Left; their military government leaders frequently favored conservative policies; and London was unable and unwilling, for various reasons, to expend further energies on Ger-

many. These factors naturally affected the battered German Left, which, in addition, revealed itself to be simultaneously irresolute and ideologically austere. To be sure, the British achieved successes that benefited the Left. They aided the reform of local government, and they gave early sanction to a form of worker participation in industry later lauded as *Mitbestimmung*. But on the whole, as Marshall concludes, the British provided less support and inspiration to the German Left than it needed to reconstruct society along nontraditional lines. Throughout, Marshall shows that debilitating daily material scarcities constantly undermined the work and the ambitions of a strong local Left. She does not say so, but it may have been too difficult to build a resonant socialism in the face of so little help from above and so much material privation below.

Regrettably, it is necessary to note the frequent technical blemishes of the book. It lacks a subtitle, and there are numerous defects such as misspellings, puzzling abbreviations, and a frequently unclear and awkward prose style. A strong editorial hand was definitely needed.

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MARIA LUDOVICA LENZI. *La pace strega: Guerra e società in Italia dal XIII al XVI secolo*. Montepulciano: Grifo. 1988. Pp. 318. L. 30,000.

This is a collection of essays mostly published in the 1980s: some are reprinted intact, some substantially changed, and two are published for the first time. The whole presents curious and confusing contrasts. Maria Ludovica Lenzi is a careful historian and both a committed pacifist and a feminist, but the commitment and the care do not always coincide. The historian is responsible for some valuable accounts of the development of military institutions in fourteenth-century Siena and an important contribution to the social history of soldiering during the formative period of the Italian wars after 1494. There is, however, a clear tension between the emphasis on horrific exploitation of an inert civilian population by rapacious soldiery in the fourteenth century and a sympathetic account of the dilemmas and dangers faced by soldiers (mostly infantry, of course) in the sixteenth century. The knightly cavalry mercenaries of the earlier period are despised as willing agents of individual warlords, whereas the mass recruits of the increasingly national armies of the sixteenth century are seen as themselves exploited by militaristic regimes. Behind both images (this book has an interesting set of illustrations) lies a view of war as avoidable violence, perpetrated by a minority of men in pursuit of their own selfish interests; the emphasis is on the emergence of militarism and military establishments committed to perpetual war.

The history of warfare is a particularly fruitful, and

dangerous, field for those in search of historical relevance. But militarism and pacifism are not eternal themes in which Italian *signori* can be equated with modern arms manufacturers or Erasmus with the Campaign for Nuclear Disarmament. The advent of *signoria* in late medieval Italy and the loss of communal liberty are not explained by the use of mercenary force as Lenzi well knows; they are primarily the result of the breakdown of anachronistic communal institutions and the growth of civic faction. There were, of course, warmongers in fifteenth-century Italy; a few of them were restless condottieri possessing relatively modest military establishments, inexperienced papal nepots with access to limited church funds, and landless political exiles; most, however, were frustrated young members of political classes anxious to hurry up the ladder of political advancement with the help of jingoistic popular support. Lenzi explores none of these possibilities; the "military establishment" of fifteenth-century Italy is not revealed at all, although she does perceive that warfare is, in fact, brought much more under control by states in which consensus within a reasonably broad political class is the essential feature. War and preparation for war had become an integral part of state building by the fifteenth century; to see it as a hideous perversion imposed by a minority of ruthless men is to miss the opportunity both to understand its real nature and to contribute to controlling its real effect.

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CHRISTOPHER F. BLACK. *Italian Confraternities in the Sixteenth Century*. New York: Cambridge University Press. 1989. Pp. xii, 321. \$54.50.

Confraternities played a central role in the social and religious lives of the laity in early modern Italy. Christopher F. Black estimates that one-quarter to one-third of adult males belonged to a confraternity at some point in their lives. Some women and children also belonged. In addition, many nonmembers were touched by the public activities of these societies. Confraternal life was extremely diverse. Some groups had only a handful of members; others, such as the Compagnia dello Spirito Santo in Naples, enrolled thousands. All cities had numerous societies, and, by the end of the sixteenth century, it is likely that nearly every town and village had some type of confraternal organization. These "brotherhoods" served varied functions, but they were above all "protective societies," offering both a promise of salvation in the next world and a measure of social insurance in this one.

Black's book is admirable on several counts. He offers a balanced view of a lay spirituality that found expression in a wide variety of forms: from private acts of self-flagellation to civic processions, from the intensive eucharistic devotions known as the *Quarantore* to pilgrimages and sacred representations. More-

over, this study is the first on early modern Italy to attempt something of a synthesis. It does not so much supplant as complement earlier, more specialized studies such as those of Brian Pullan on Venice and Ronald Weissman on Florence. Along the way, Black succeeds in demonstrating how difficult it is to generalize about confraternal life. Nonetheless, he does suggest ways in which the history of confraternities dovetails with the major initiatives of both the Catholic Reformation and the Counter Reformation. By the end of the sixteenth century, for example, it is likely that lay confraternities had given up some of their autonomy and were increasingly under clerical control.

The issue with which Black is most concerned, however, is Catholic philanthropy. He argues forcefully against analyses that interpret confraternal charity in purely economic or social terms. Black does not deny the role of economic dislocations that all too frequently reduced the living standards of many Italians. But his emphasis is on capturing sixteenth-century motivations. Acts of charity, he maintains, were fundamentally religious. Members of confraternities were as concerned with the spiritual needs of the recipients as with their physical deprivations. His discussion of the vocabulary used to describe the poor is especially effective in demonstrating how differently the problem of poverty was viewed in the early modern world. This argument is a useful corrective to some of the claims of historians who have seen the new philanthropic initiatives of the period almost exclusively as instruments of social control. But Black may overstate his case; surely it is legitimate for scholars to examine consequences as well as stated intentions in order to elucidate the values and assumptions of past times.

This is a rich and highly informative book. It ranges broadly from questions of internal institutional organization and finances to the role of confraternities as patrons of art and music. Black's thorough grasp of the secondary works as well as his own perceptive use of archival materials from Perugia, Bologna, and Venice provide not only a welcome survey of the current state of scholarship but also several valuable suggestions about possible directions for research.

JOHN MARTIN
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PATRIZIA AUDENINO. *Un mestiere per partire: Tradizione migratoria, lavoro e comunità in una vallata alpina*. (Collana dell'Istituto di studi storici Gaetano Salvemini di Torino, number 4.) Milan: Franco Angeli. 1990. Pp. 281. L. 32,000.

Interdisciplinary understanding of Italian emigrants is enriched by Patrizia Audenino's detailed study of the Alpine valley of Cervo. The people of Cervo

could not forge a living from the land's meager resources. For centuries women worked the difficult terrain and men emigrated annually to mining and construction sites. The "perfect" immigrant was frugal, industrious, individualistic, sober, chaste, and peaceful; he wrote home frequently and included remittances whenever possible. After about forty years of seasonal migration, he accumulated enough to return permanently and live out his remaining days at leisure. The *siunèra*, a name derived from the herbs that the women gathered, was year-round manager of all of the family resources; she pastured a cow or two, foraged for nuts, hauled and chopped wood, grew vegetables, and raised children.

The men needed the women more than the women needed the men. Widowers invariably remarried, often within months, but widows generally made do on their own, even when responsible for young ones, or else they went to live with a relative. Even then they retained economic independence.

Male migration patterns became clear in the eighteenth century when a boom occurred in the construction and repair of fortifications after the Treaty of Utrecht. Leading valley contractors made huge profits by relying on local skilled workers ready to put in long days from March through November. These proto-capitalists were neighbors, linked through networks of godparents and entrusted to take care of the family of anyone killed or maimed on the job even in the absence of any written obligation. In the nineteenth century, similar labor networks found ample work in railroad construction and in the rapid urban development of northern Italy. In the 1870s, village elites even financed state-approved schools where young men might learn enough engineering to earn a diploma after five years of winter school, allowing studious artisans to become professionals.

In the twentieth century, this annual, seasonal migration pattern broke. The men were in Bolivia, China, West Virginia, and the Congo—too far to return every winter. Work was now available all year, making it possible to save even faster and return home sooner in the long run. Young married women unwilling to continue the hard life of a lonely *siunèra* preferred to accompany their husbands and find some sort of work in a distant country. If the family came back at all, they were more likely to buy an apartment in Biella, with schools for their children and medical care for themselves. Medical care was important, for these were the men victimized by silicosis, especially those who had worked in New Hampshire and Vermont, where the type of granite they mined and the technologically advanced drills they used doomed them to an early death. As with most of Italy's mountain settlements not favored with tourism, those of the Cervo valley are now moribund.

The range of documentation for the study and its intelligent use are outstanding, an exemplum and a

challenge for American scholars planning to embark on similar research.

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GIORGIO SPINI. *Risorgimento e protestanti*. (La Cultura, number 91.) 2d ed., rev. Milan: Saggiatore. 1989. Pp. 458. L. 60,000.

The present study is a revised and expanded version of the volume of the same title published by Giorgio Spini in 1956. Like the first edition, the second examines the *Risorgimento* from the perspective of nineteenth-century Protestantism in the peninsula and abroad. Spini has published extensively on the interrelationship between religion and politics in European and American life and has taught in the United States as well as in Italy. In his study he uses both recent historiography and fragments of his earlier scholarship to assess the impact of Protestantism on *Risorgimento* thought and action. The resulting work is much more than a modest synthesis of what has previously appeared on the subject, providing a thoughtful analysis of the role of Protestantism in the unification process and the resurgence of the Italian nation.

The book is divided into eleven chapters; the first three outline the Protestant position in the peninsula and the broader European world in the eighteenth century. Here Spini examines the Protestant contribution to the emergence of the Italian conscience, emphasizing the Waldensian role in Piedmont, the part played by the non-Catholic foreign colonies in the major cities of the various Italian states, as well as the French, Swiss, and English influence in the formation of liberal and national thought in Italy. The part played by the Jansenists, the Waldensian community (sometimes dubbed the Israel of the Alps), and the agencies of the various Evangelical churches and bible societies is explored.

A number of chapters chronologically trace the Protestant influence in Italy during the revolutionary and Napoleonic age (chap. 2), the age of the Restoration (chap. 4), the eve of the revolutionary upheaval of 1848–49 and the subsequent Second Restoration (chap. 7), and finally the quest for Rome (chap. 11). In chapter 10, Spini sheds light on a forgotten aspect of the *Risorgimento*, namely, the Protestant awakening that accompanied the political reorganization. In chapters 8 and 9, he focuses on the laicization of the Sardinian state and its impact abroad, particularly in England, where the myth emerged that Italy was in the midst of a Protestant reformation. He perceptively delves into the role played by English Protestants in molding their country's public opinion and determining their country's diplomatic stance toward the Italian unification engineered by Camillo Cavour. Readily acknowledging that the Italian Protestants

represented only a tiny fraction of the peninsula's population, Spini shows that the moral and philosophical influence of the Protestant world exercised considerable influence on the course of Italian developments in the nineteenth century.

This study is a work of meticulous research and mature scholarship. Spini weaves together facts and analysis to pose interesting questions while imparting important insights. Based on a wide variety of primary sources and secondary works, catalogued in the concluding bibliographical essay (pp. 387–437), this book confirms the author's preeminent position among historians of Protestantism during Italian unification. The specialist will appreciate the painstaking and exhaustive research and comprehensive coverage, although the general reader might be overwhelmed by the wealth of information and scholarly digressions. Nonetheless, this remains an original and thought-provoking study that will have to be consulted by all those who seek to assess the contribution of Protestantism to the Italian *Risorgimento*.

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GIORGIO MORI, editor. *Prato: Storia di una città*. Volume 3, *Il tempo dell'industria (1815–1943)*. Part 1, *Città e territorio, popolazione ed economia*. Florence: Comune di Prato or Le Monnier. 1988. Pp. xiv, 659.

GIORGIO MORI, editor. *Prato: Storia di una città*. Volume 3, *Il tempo dell'industria (1815–1943)*. Part 2, *Società, cultura e politica*. Florence: Comune di Prato or Le Monnier. 1988. Pp. 663–1563.

The merchants of Prato, whose medieval prowess Iris Origo made famous, have continued to prosper for another six centuries. The persistence of this industrious little city—ever adapting to changing technology, markets, taste, and politics while remaining proudly (and a little resentfully) distinct from nearby Florence—raises interesting historical questions, a point given the imprimatur of Fernand Braudel in the extraordinary project of which these volumes are a part. He chaired the committee of researchers (more than sixty of them, principally from the universities of Florence, Pisa, and Siena but also from the universities of Bari, Ferrara, Genoa, and Rome, among other institutions) who systematically searched through archives, conducted seminars, and eventually contributed chapters to a history of Prato from its origins to the present. The first volume went up to 1494; the second covered the period from then until the defeat of Napoleon. Now we have the third volume, on "the industrial era," from 1815 to the fall of fascism in 1943 (although the treatment becomes notably thinner for the period after World War I). This volume, Braudelian in its affection for detail (nearly a million words in two parts, each a handsome, thick book) and its emphasis on economic

structures, makes some reference to its predecessors but essentially stands alone. So to a considerable extent do its sixteen chapters, each by a different author.

The product of a carefully coordinated team, its members familiar with each other's findings and working from a common agenda, the project is unusual in more than scale. The framework throughout is analytic, and every page reveals intensive research in previously unexplored original sources. Conceived more as a case study of modern social change than as just another local history, it is nevertheless as much a monument to Prato (and to the civic pride and prosperity that subsidized it) as to current scholarship. Four hundred well-chosen and beautifully reproduced illustrations, many in color, add significantly to the text and to the sense that this is, after all, the kind of public exposition in which Italian towns excel and which cities far more famous might envy.

So many purposes, so much new data shared, and so many hands are not easy to coordinate. The study's coherent structure is an organizational achievement, visible in chapter topics and, within chapters, in a similarity of approach that adopts a rhetorical solution for combining the concerns of international historical scholarship with the piles of facts dropped from the shovels of scores of diligent researchers. Familiar large historical themes introduce each section and are continued in many of its paragraphs; these are then interwoven, often very skillfully, with nicely chosen quotations from historical sources, stirring local flavor into European trends. All of that is followed by an affectionate consideration of the events, institutions, groups, and individuals of Prato.

Despite a greater sense of distance and of irony, these studies differ from the tradition of fond local studies less in tone than in principle of selection. Not the needs of narrative for descriptive background and sequential development but the promise of science and respect for research determine what is included here. Prose serves as the setting for facts, facts conceived as data for implicit comparisons and facts as the fruits of extraordinary team research—the events with their precise dates and the names of participants, institutions with their budgets and changing membership, groups with lists of their activities, individuals with their occupations, family ties, and influential connections. Numbers appear on nearly every page: the number of straw hats produced and of bobbins spinning thread; the prices of food, products, and tickets; the size of families, memberships, crowds; the kilometers of roads and railways; amounts imported and exported; profits and debts. Usually such data are given for several different years, suggesting possible trends even when not full enough to prove the point or sufficiently similar in date and kind for systematic comparisons. Such research nourishes the need for more; and before each tiny capillary can be fed into a larger stream, its incompleteness is duly noted, a gesture to

the modesty of science and the insatiability of the need to know.

This works better in the first part than in the second, the volume on society, culture, and politics, which even when treating festivals, religion, or ideology tends to privilege hard statistical data over ideas and symbols. The chapters here establish impressively that Prato took part in every trend and movement of modern history, evidence of the vitality of local life and a significant reminder of the scale on which the great historical processes—of industrialization, politicization, nationalist mobilization, social conflict, and cultural change—took place. Although the scale is microscopic, the view through the microscope is from the top down. Issues are thus introduced and conceived in the conventional terms of good historical surveys, and all of this research does little to alter our understanding of them. The emphasis is institutional, stronger on markets, hospitals, and church than on custom or belief. Interesting data on the programs, size, and decline of labor movements are only modestly revealing of how workers saw themselves, why they combined or were overwhelmed, what they wanted, or how they felt. There is a lot to learn here about the famous fairs, theaters, schools (including the prestigious Collegio Cicognini), charities, and civic life of Prato but surprisingly little explanation of what sustained the remarkable level and special character of these activities beyond the inevitable ready-made references to Prato's Ghibelline traditions and suggestions that nobles were few and inclined to cooperate with the middle class and that the lower middle class was unusually prominent.

The heart of this project lies rather in the first part, the volume on industry, agriculture, property, demography, roads and railroads, and urban geography. Here the tables, graphs, and marvelous maps that punctuate both volumes integrate magnificently with comprehensive discussions to establish an awesomely detailed description of economic development, landholding, sharecropping, the ties of city to countryside, family size, housing, and urban infrastructure as Prato grew from twenty-four thousand inhabitants in 1810 to seventy-four thousand in 1940. There is more information in these pages than any single reading can comprehend, more indeed than anyone, even a census taker or real estate developer, could want to know. Nevertheless, so solid an achievement can in time make a broader contribution to historical understanding. Scholars treating any of these topics in nineteenth-century Italy or Europe can find helpful bench marks, useful comparisons, and daunting challenges in this exhaustive analysis of Prato. At the very least, this systematic exploration of archives has produced another kind of archive—in print, portable, cross-checked, carefully assembled, and comprehensibly organized. Anyone who has ever waited for a document to be found or poured over

dim writing on scattered bits of paper has reason for respect and gratitude.

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MARTA PETRICIOLI. *Archaeologia e Mare Nostrum: Le missioni archeologiche nella politica mediterranea dell'Italia, 1898–1943*. Foreword by SERGIO ROMANO. Rome: Valerio Levi. 1990. Pp. xxi, 442. L. 30,000.

Marta Petricioli's book possesses all of the elements for a first-rate Agatha Christie thriller. Worldly archaeologists such as Federico Halbherr and Gaetano De Sanctis and top diplomats such as foreign ministers Antonino Di San Giuliano and Dino Grandi people her pages. The story unfolds against a background of colorful sites from Iraq to Crete to Albania. As Petricioli shows, the archaeologists were scientists, absorbed in their task of reconstructing the past. But they were also men, subject to patriotic fervor, professional jealousies, and material self-interest—including a need for subsidies to finance their projects. At times these passions swept them into the murky waters of foreign policy. In some cases, they became, as Petricioli puts it, the "cavalry advance scouts" of imperialism.

Like any good thriller, the intrigues of the archaeologists sometimes led to fatal consequences. The American archaeologist Herbert Fletcher de Cou, for example, was mysteriously murdered by a group of Bedouins in Cyrenaica in March 1911. Italian archaeologists were behind it, the Americans claimed. Desperate to neutralize an American expedition at Cyrene, the Italians had bribed the Bedouins, according to the Americans. An Italian investigation corroborated by the British denied any responsibility. Nevertheless, Italian hopes for an archaeological project in Cyrenaica were crushed. "It will be necessary to conquer by means other than peaceful penetration" (p. 137), Halbherr wrote prophetically to Di San Giuliano. Four months later, the Italians landed in Tripoli.

By and large, Petricioli shows that science and politics formed a stormy partnership. Her period of study—from the turn of the century to the collapse of fascism—reveals frenetic activity on the eve of World War I and the early 1930s, followed by hard times when the hostility of host nations, slim budgets, and new twists in Italian foreign policy stymied scientific work. One of the main reasons for the ups and downs, Petricioli indicates, is that archaeology was only one of many instruments that the Italian government used to spread its influence throughout the Mediterranean and the Middle East. Cultural institutes, hospitals, and religious, geographical, and scientific missions jostled with the archaeologists for government funds.

Petricioli has done her archival work thoroughly.

She has delved into the records of learned societies and archaeological expeditions, the Italian, British, and French foreign ministry archives, and the private papers of top archaeologists such as Halbherr and De Sanctis. Her work could have contained some even more fascinating reflections if she had used papers of some of the host countries such as the Ottoman Empire, Iraq, or Albania—not an easy task these days.

Ironically, Petricioli's archival work is both a strength and a weakness. She allows her wealth of material to dictate her story too often. She is so busy transcribing her notes that she does not stand back to consider what they mean. For example, only in passing do we find out about Halbherr's background, and we learn nothing of his physical appearance. We do not understand the institutions and the techniques of archaeology and how they evolved during the period she is studying. Finally, the author could have reflected more on the symbolic value of archaeology. As the historian and diplomat Sergio Romano points out in his introduction, archaeology was a two-edged discipline. It was a symbol of the highest standards and aspirations of Western civilization and science. At the same time, with its reflections of past glories, the discipline became a tool of aggressive, hysterical nationalism. Despite its flaws, Petricioli's study is a fascinating and entertaining contribution to the study of the imperialist process.

CLAUDIO G. SEGRE
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EMANUELA SCARPELLINI. *Organizzazione teatrale e politica del teatro nell'Italia fascista*. (Pubblicazioni dell'Istituto di Storia medioevale e moderna dell'Università di Milano, number 9.) Florence: La Nuova Italia. 1989. Pp. xvi, 385. L. 44,250.

Emanuela Scarpellini has produced an excellent study of fascist policies toward the theater, beginning with post-World War I Italy and concluding with the collapse of the Saló republic in 1945. She has made good use of sources in the archives and a wide range of newspapers and periodicals from the period. The chronological organization works well, as the author takes us through the various stages of fascist policies toward the theater, which were, in turn, part of the regime's overall policies with respect to culture, censorship, propaganda, control of the media, and fulfillment of the fascist "revolution."

Benito Mussolini proclaimed in 1926 that fascism should create a new art for the times, a "fascist art." Instead, his government pursued rather traditional types of censorship to preserve public decency and to placate the Catholic church. Efforts were made to ensure that productions of Italian plays outnumbered those of foreign origin. The regime welcomed Luigi

Pirandello's support and employed such playwrights as Anton Bragaglia and Giovacchino Forzano to further its policies.

The turning point came in the next decade when both the depression and the advent of cinema undermined traditional theater just at the time that the regime became totalitarian in aspiration. Thus, the first corporation formed as part of fascism's corporate state was one for theater in 1930, the *Corporazione dello spettacolo*. It continued as a separate corporation after the creation of a total of twenty-two corporations in 1934.

Scarpellini's discoveries and conclusions are not surprising and are, in fact, consistent with what others have found in studying fascist policies toward cultural activities. The regime sought increasingly to dominate theater, opera, and cinema in the 1930s, creating the Ministry of Popular Culture (*Minculpop*) in 1937. The author finds that even though fascism may not have created an effective foundation for consensus between regime and people, its efforts brought about a general acquiescence that benefited the regime.

In reaching out toward the people, fascism tried to exploit professional theater, opera, and amateur theater groups for its own ends. One of its innovations, the Thespian Cars (*Carri di Tespi*), proved highly popular. These caravans brought theater to provincial and rural audiences. At each stop, workers set up an outdoor theater that could accommodate thousands of spectators. Similar caravans for cinema followed. The leisure-time organization known as the *dopolavoro* sponsored these shows, and Scarpellini's account is consistent with that of Victoria De Grazia in her *Culture of Consent* (1981), a study of the *Organizzazione nazionale del dopolavoro*.

Scarpellini points out one of the many contradictions of the fascist regime in commenting on the work of the *Corporazione dello spettacolo* in the early 1930s. The very existence of an organization that included, even in a limited way, a role for representatives of a profession undermined the aspiration for totalitarian control. That drive for greater control manifested itself in the incorporation of the *Corporazione dello spettacolo* in the Ministry of Press and Propaganda and then the *Minculpop* apparatus. Thus, the regime tried to resolve the contradiction but with mixed results in Scarpellini's judgment.

The author does take seriously the fascist drive for totalitarian control and its express wish to fulfill its revolution by creating a new culture and a new Italian. It fell far short of the mark, but she argues that fascist policies toward theater and culture in general only make sense when seen within the context of these larger ambitions.

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ROLF FISCHER. *Entwicklungsstufen des Antisemitismus in Ungarn 1867–1939: Die Zerstörung der magyarisch-jüdischen Symbiose*. (Südosteuropäische Arbeiten, number 85.) Munich: R. Oldenbourg. 1988. Pp. 206. DM 68.

How important was anti-Semitism as a social and political force in modern Hungary? The Tiszaeszlár affair of 1882–83 represented the first ritual murder trial to take place in a modern European state. Although the trial ended in the acquittal of the defendants (there were fifteen in all), the episode emboldened individuals such as Gyozo Istöczy to establish Hungary's first anti-Semitic political party, which during the following decades enjoyed greater electoral success than similar parties in Germany. Yet, as Rolf Fischer convincingly argues in this authoritative monograph, until World War I organized anti-Semitism in Hungary represented the voice of a relatively small minority. Even in the last decades of the Dual Monarchy, when the anti-Semitic movement succeeded in extending its social and political appeal and partly in defining the programs of both political Catholicism and the agrarian movement, it never was able to affect state policy toward Hungary's Jews.

The Hungarian state's relations with its Jewish community—which numbered over 911,000 in 1910, representing 5 percent of the overall population—proceeded from a liberal national consensus that operated in the context of relative economic backwardness. This consensus held that the Magyar political class, largely gentry, would enjoy dominance in Hungary's multi-ethnic society and invite middle-class Jews and others to take up careers in the liberal professions and to develop the country's commercial and industrial sector. The crucial cultural component in all of this consisted of Magyar nationalism's "inclusive" self-definition, through which it encouraged the affiliation of non-Magyars to the nation as the result of linguistic and political acculturation. Fischer effectively argues that neither aristocratic conservatism nor agrarianism, neither political Catholicism nor anti-Semitism, seriously challenged this foundation of the Hungarian state after 1867. Until World War I, anti-Semitism took the form of a "political argument" with the status quo but never threatened the social and political order.

Hungary's shared defeat in the First World War, followed by the revolutionary and counterrevolutionary upheavals of 1919, marked in Fischer's view a decisive turning point. After this time, all of the country's conservative and counterrevolutionary groupings were united in the conviction that Hungary's policies had to follow a "Christian-nationalist" course. The term itself implied a firm rejection of Hungary's liberal vision of the last half-century by ascribing to Jews—though linguistically and culturally Magyar—the status of national aliens. (The size of Hungary's Jewish community after the Treaty of Trianon was cut by almost half, to 473,000, but its

relative weight within the population at large rose to 5.9 percent.)

Fischer's main argument is that this drastic revision of political and cultural definitions found acceptance in state policy. Hungary after 1919 promoted an "official anti-Semitism" that had been totally absent—perhaps even unimaginable—in the prewar, multinational kingdom. The *numerus clausus* law of 1920 aimed at reducing the presence of Jews in institutions of higher education and the bureaucracy. It inaugurated an era of state-sanctioned discrimination (what Fischer calls "isolation and dissimulation") and introduced the concept of "race" to Hungarian law for the first time. In 1932, Gyula Gömbös, head of the anti-Semitic faction within the ruling coalition, assumed the office of prime minister. Although he failed to translate his earlier pronouncements on Jews into formal legislation, he did cultivate close relations with National Socialist Germany and spoke of creating a Berlin-Rome-Budapest axis of fascist states.

Ultimately the Austrian Anschluss and the Munich accords provided the main impetus to the renewal of anti-Jewish legislation in Hungary. The "First Jewish Law" of May 1938 extended Jewish quotas to the free professions and larger business enterprises. It represented a response to the increasing calls from within Hungary to restrict Jewish participation in national life, and it enjoyed widespread popular support. The "Second Jewish Law," dated May 5, 1939, betrayed the unmistakable influence of German racial anti-Semitism and derived at least partly from pressure from Nazi Germany to conform to its standards on the "Jewish Question." This legislation carried a detailed definition of who was a Jew and prohibited certain occupations altogether to Jews for the first time since 1867. And it, rather than the so-called Racial Protection Law of 1941, marked the start of Hungary's "capitulation" to Nazi-style persecution. Hungary moved in the direction of German models and German intentions, according to Fischer, not only because of foreign policy considerations but also as the result of an internal political dynamic that had begun two decades earlier.

Though a sound piece of scholarship, Fischer's book reflects the limitations of his source material. The work is based mainly on contemporary periodicals and other printed sources; he reveals very little of the actual decision-making process of Hungarian lawmakers and high-level bureaucrats; and the reader does not learn what interaction, if any, took place between the government and the Hungarian Jewish community. Fischer's analysis, moreover, is greatly indebted to the work of other scholars of Hungarian and Hungarian-Jewish society, scholars such as T. Ivan Berend, William O. McCagg, Andrew Janos, Randolph Braham, and Viktor Karady. It is not clear to me that Fischer's thesis differs significantly from what the work of these individuals would already have led one to expect. What Fischer has done is to isolate and trace a single theme in Hungar-

ian politics over seven tumultuous decades, to offer a clear and readable account, and to remind us of where the disjunctures in Hungarian history really lie.

HILLEL J. KIEVAL
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ROBERT S. WISTRICH. *The Jews of Vienna in the Age of Franz Joseph*. (The Littman Library of Jewish Civilization.) New York: Oxford University Press. 1989. Pp. xiv, 696. \$79.00.

In this fascinating book Robert S. Wistrich relies quite as egregiously as did Carl Schorske, in a famous essay some years back about Vienna, on musical organization. The book falls into four symphonic movements, of which the first (in six parts) introduces Vienna's Jewish Community: its *Gemeinde* institution up through 1848, its swelling population up to 1900, its philanthropic elite up past the immigration of the *Ostjuden*, its three great preachers, the great Jewish liberal politicians, and finally (maestoso) the defenders of the community in the 1910 era. In his second movement Wistrich goes back to 1873, introduces racial anti-Semitism, and thereupon in four chapters, with mounting passion, recounts the articulate responses to it by *Gemeinde* preacher Adolf Jellinek, by suburban Rabbi Joseph Bloch, and by the newly founded *Österreichisch-Israelitische Union*—up to about 1900. The third movement again backtracks. It introduces Zionism, first with student violins from the 1880s, then with the versatile flute of Nathan Birnbaum from the 1890s, then with Theodor Herzl's pure trumpet, finally with a quartet of Jewish critics—up to about 1908.

The book's concluding and most obviously contrapuntal movement opens con brio with a discussion of Karl Kraus and Otto Weininger, the two most anti-Jewish of Vienna's Jews. There follows an essay on Sigmund Freud, whom Wistrich calls, because of his strength of mind, "the most un-Viennese of all Vienna's Jews" (p. 582). Then comes Arthur Schnitzler, the most self-confident of them all. And the final chapter, labeled "Imperial Swan Song," presents Hermann Broch, Hugo von Hofmannsthal, Gustav Mahler, Arnold Schoenberg, Richard Beer-Hofmann, Martin Buber, Stefan Zweig, and Josef Roth—a Jewish requiem from the period of the Habsburg collapse.

Wistrich's work is both encyclopedic and brilliantly researched and beyond question will remain, far longer than several other recent productions concerning turn-of-the-century Viennese Jewry, both interesting to general readers and useful to scholars. His occasional errors are of the order of calling Isaac Loew Hofmann von Hofmannsthal the offspring of a "poor immigrant Bohemian family." (Hofmann actually came to Vienna as son-in-law to the wealthy Baruch family of Prague—but who cares?) An effec-

tive index even makes it easy to locate whatever one wants within this vast text.

But Wistrich's symphonic organization of his material, like Schorske's waltz, remains a trick, and one should recognize its effects. Above all, it releases his strong literary and musical priorities from conventionally chronological political and social history. Despite masses of data, this is *Geistesgeschichte*, and characteristically we do not find out what specifically happened to the Jewish society of Vienna at the end of Franz Joseph's "Age," during the Great War. Further, although the author very cleverly keeps his vast material in order by using the rabbis Jellinek, Bloch, and Moritz Guedemann as literary-musical leitmotifs, one should be aware that he is arbitrary in his choice of them for study and in other choices, too. How many of the books about the Viennese Jewish intellectuals include these rabbis, for example, while omitting the socialists, as this one does? How come Mahler, an absolutely pivotal Franz-Josephian Jew, pops up only at the very end, among the post-Habsburgians? How come von Hofmannsthal is present, while Ludwig Wittgenstein, who had much purer Jewish "blood," is out?

And the symphonic organization makes possible a certain evasion of important scholarly controversies. For example, Andrew Handler has recently demonstrated that Herzl was much more "Hungarian" than is usually admitted. One would hope that scholars would now at least peek at the founder of Zionism in the context of other Hungarian-Jewish adventurers such as Arminius Vambery and Lincoln Trebitsch, yet Wistrich, whose symphony is about Vienna, just waves that possibility aside, as too discordant (pp. 426–27). His treatment of the Freudian debate is similar. Peter Gay has recently argued on the basis of the whole oeuvre that Freud was European, not "just Viennese." Wistrich documents most effectively the intensity of Freud's lifelong Jewish feeling. But meanwhile, with hardly a mention of the oeuvre, he pronounces melodiously: "The inherent duplicity of their situation produced for a whole generation of . . . young Viennese Jews . . . the kind of localized neuroses out of which Sigmund Freud was eventually to construct his universalist psychoanalytic typology" (p. 541). And it is not with data but through the sheer harmony of this contextual reference that he makes his case for a Viennese Freud. He shoots down Gay in a brief footnote (p. 558).

These are minor flaws in an important book.

WILLIAM MCCAGG
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RICHARD S. GEEHR. *Karl Lueger: Mayor of Fin-de-Siècle Vienna*. Detroit: Wayne State University Press. 1990. Pp. 408. \$29.95.

Students of modern European history have been well aware of the influence on Adolf Hitler of two major

fin-de-siècle Viennese political figures, Karl Lueger and Georg von Schönerer. In *Mein Kampf*, Hitler held up Lueger as the model charismatic leader of a mass movement; he admired von Schönerer for his elaboration of racial anti-Semitism. Now that the *fin-de-siècle* twentieth century evinces ever greater interest in, and fascination for, the nineteenth-century *fin de siècle*, historical scholarship about that earlier period is blossoming. In *Fin de Siècle Vienna: Politics and Culture* (1980), Carl Schorske devoted most of one chapter to these two fascinating actors on the Austrian political stage. We now have a detailed sociopolitical study of Lueger by Richard S. Geehr, who previously edited and translated Lueger's papers, "*I Decide Who is a Jew!*" *The Papers of Dr. Karl Lueger* (1982).

In this elegantly produced and lavishly illustrated book that could have benefited from a more effective editorial hand, Geehr presents a full-scale biography of Lueger set in the historical context that gave rise to a militant petite bourgeoisie and its charismatic spokesman. In the first three chapters, Lueger's career prior to his accession to the mayoralty of Vienna receives careful and extended attention, ranging all the way from the education of the boy at the hands of his father and the loving attention of his mother to the bitter conflict over receiving imperial assent to assuming the office to which he had been elected over the stubborn resistance of Emperor Francis Joseph I.

In the succeeding chapters, Geehr focuses on the major dimensions of Lueger's lifework. We witness Lueger presiding over the modernization of Vienna as the imperial capital city in an era in which rampant private entrepreneurship existed side by side with what is sometimes called "gas and water socialism." If I were to mention Lueger's efforts to ensure that Vienna had an ample supply of *Hochquellenwasser* (mountain spring water), it would only be because this remained the pride of the city in my youth, long after Lueger's term of office and after his death. In a fascinating chapter Geehr pays special attention to Lueger's relations with women, not just in the sense of Lueger as "*der schöne Karl*" but because of the significant role that women played in the Christian Social movement.

In still another chapter the cultural dimensions of Lueger's politics and political life are treated with considerable skill. Although Lueger's political life started in the liberal camp, his political career represented the triumph of a Catholic-conservative culture. But the cultural scene of *fin-de-siècle* Vienna was more complex than such a liberal-conservative dualism would suggest. Imperial loyalties conflicted with Pan-German nationalism, and anticlerical secularism contested with Catholic pieties and the demands of the Roman Catholic establishment.

Inevitably, a separate chapter is devoted to Lueger's anti-Semitism. Here the author's utter commitment to the canons of scholarship seems to produce a little too much of an "on-one-hand-on-the-

other-hand" effect. There might well be evidence that Lueger's anti-Semitism was an exercise in pure political cynicism, that Lueger did not share von Schönerer's racial anti-Semitism, and that Lueger genuinely admired the Jews' devotion to their religion—the note on which Geehr closes the chapter on anti-Semitism. In the light of the Austrian record as fashioned by Lueger and since his time, a more unequivocal judgment might well have been in order.

Having dealt skillfully and in expansive detail with Lueger's life and works, Geehr puts us further in his debt by devoting a concluding chapter to the bibliography on Lueger, that is to say, a critical assessment of the biographers who have examined the life and career of the *schöne Karl*, usually with quite specific objectives in mind to praise or blame, mostly to blame. Although much of the early biographies were exercises in idolatry, later writers, especially those after World War II, have hewed to more accepted scholarly standards. Yet even this group bears the imprint of *parti pris*, for they tend to minimize, if not omit entirely, the anti-Semitic dimensions of Lueger's life and work.

Fin-de-siècle Vienna continues to fascinate not only hordes of tourists but also historians, writers, and scholars of various stripes. Yet neither tourists nor writers have much interest in the visible and demonstrable achievement of socialist Vienna of the interwar years, especially the period from 1918 to 1934. Vienna under the leadership of the Social Democratic party became a pioneer welfare state in providing workers' housing, modern schools and libraries, well-baby clinics, and universal prenatal care, among other things. Yet it would seem that decadence is more "interesting" than—if "cadence" is not the proper word, what would it be? But whatever the word would be, such an unbalance has puzzled me, and I am still waiting for a satisfactory answer.

ALFRED DIAMANT
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KEVIN MCDERMOTT. *The Czech Red Unions, 1918–1929: A Study of Their Relations with the Communist Party and the Moscow Internationals*. (East European Monographs, number 239.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1988. Pp. xiii, 350. \$40.00.

For obvious reasons Czech Communist historiography after 1948 devoted a great deal of attention to the interwar history of the Communist party of Czechoslovakia (KSČ) and to the Red, or Communist controlled or affiliated, Unions. Although significant work on the KSČ has also appeared outside Czechoslovakia, Kevin McDermott's book is the first systematic study of the Czech Red Unions to be published in the West. It is based largely on an extensive and

skillful use of a vast amount of archival materials that were available to him in Czechoslovakia.

The author provides a brief, but useful, survey of the origins and development of the Czech Social Democratic trade union movement (OSČ) under the Austro-Hungarian empire and then, in a more comprehensive manner, details the schism within the OSČ after World War I and the formation in 1922 of the Communist trade union organization (MVS) as well as the so-called independent Red Unions. The rest of the book, more than half of it, focuses in a very detailed fashion, too detailed in fact, on the relations between the OSČ and the Red Unions, on the one hand, and among the Red Unions, the KSČ, the Comintern, and the Profintern, on the other. He concludes with the bolshevization of the KSČ and the split in the MVS in 1929, which followed the Comintern's abandonment of the "united front" tactics in favor of the hard, "class against class" line.

McDermott's aim is to use the case of the Czech Red Unions to challenge the view that in the 1920s Joseph Stalin and the Soviet leadership of the Comintern and the Profintern "increasingly controlled all aspects of the world communist movement" (p. xi). One could question his claim that this view is "widely accepted in the West," since most students of international communism would probably agree with him that Stalin did not succeed in imposing on it "centralization, monolithism and uniformity" before 1929 (p. xi). He does argue convincingly, however, that during the 1920s the KSČ was not of one mind in its attitude toward the internationals and, hence, did not speak with one voice to the Red Unions. And, furthermore, although there were divisions among the leaders of the Red Unions, the leadership remained in the hands of those who were determined to fight the interferences from both the KSČ and Moscow and succeeded, at least to a certain extent, in safeguarding the independence of the unions until 1929. He explains the leadership's resistance to the greater centralization and discipline demanded by Moscow and its espousal of "wider autonomy and a measure of national specificity" (p. 236), "the seeds of national communism," by pointing to "the Czech democratic and pluralist traditions" (p. 246). Unfortunately, however, and this will be of concern especially to those of his readers who are not well acquainted with Czech history, there is hardly anything in the book about these traditions or about the democratic system of the First Czechoslovak Republic in the context of which the Red Unions originated and functioned freely.

All in all, however, McDermott's well-researched and clearly written book makes a valuable contribution to our understanding of the Czech labor movement and especially of the inner workings of international communism in the 1920s.

ANDREW ROSSOS
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H. GORDON SKILLING. *Samizdat and an Independent Society in Central and Eastern Europe*. Columbus: Ohio State University Press. 1989. Pp. xi, 293. \$30.00.

When Czechoslovak television reported on the first joint gathering in March 1990 of the members of Charter 77, a human rights movement organized by Václav Havel and others in 1977, the camera briefly showed Havel in the president's box. Alongside him was H. Gordon Skilling. Although Skilling later reported that they carried on no conversation, their placement together carried symbolic value. Skilling had been the chronicler of this opposition movement, had interviewed its leaders, read its publications, and been detained by the police.

Skilling has been present at the great crises in twentieth-century Czech history: as a student in the gathering Nazi doom of the late 1930s, during the Communist putsch of 1948 and during the Prague Spring in 1968. As a political scientist and historian, Skilling has had a knack for seeing the future, or at least understanding the present and seeing the correlation of forces in the future. He and Franklyn Griffiths recognized the emergence of interest group politics in Communist societies when most other people still saw monoliths. He chronicled and analyzed the development of the "interrupted revolution" of 1968.

This book does not measure up to his earlier accomplishments. Although its title suggests broad geographical application, in reality it is focused on the Czechs. This specificity is significant because the role of independent communication was far more important in Poland than in Czechoslovakia or Hungary in the development of a civic society. In Poland the independent system of communications (the "second circle") became more consequential even than official publications and blended together with a growing legal opposition.

Skilling's goal is to analyze the phenomenon of the independent society in the evolution of Eastern Europe. He begins by providing a description of the historical evolution of samizdat, arguing that it served as a vehicle of expression for the maintenance of national cultures and individual integrity and helped channel political dissent and opposition. Symbolically this broke the Communist monopoly of information, but its influence ought not be overestimated. Those who read the independent press, or listened to Radio Free Europe, had already lost faith in the Communist system. The growth of readership and listenership measured the decline of Communist influence and power, not the influence of the independent press. Skilling only reluctantly admits that, even within the declining Communist system, there were individuals who quietly promoted the development of a humane democracy, stretching the boundaries of communism and hoping they would eventually break. Time ran out on them when the people took to the streets in Leipzig, Prague, Bratislava, Timișoara, and else-

where. The reluctance of the new regimes to accept the inside liberalizers as allies is complicating the consolidation of the new systems.

The heart of the book is a chronicle of the activities, especially publication, of the different strands of the small Czech dissident movement. Skilling surveys many of the publications, discusses their contributors, and analyzes the way in which they played their politics. He devotes one of his nine chapters to the evolution of the historical profession and the emergence of independent historiography. Despite the quiet assistance of some nondissident historians, the independent historians were denied access to the full range of historical sources necessary for fully professional work, but Skilling shows that the value of their writing lies in addressing topics declared off-limits by the regime.

This book, although not Skilling's best, is valuable nonetheless because, like journalism, it is a first draft of history. It provides a descriptive framework of dissident activities in Czechoslovakia, which future historians can fill out, critically evaluate, and place in the context of developments in the official sphere.

OWEN V. JOHNSON
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MARY ELLEN FISCHER. *Nicolae Ceaușescu: A Study in Political Leadership*. Boulder, Colo.: Lynne Rienner. 1989. Pp. x, 325. \$42.50.

In her political biography of Nicolae Ceaușescu, Mary Ellen Fischer is thorough and appropriately subjective, introducing the reader to both his personality and his policies, his background as a peasant and as a first-generation revolutionary. Fischer thereby seeks to explain his leadership from the perspective of individual character and temperament.

Fischer details the traits of his rule that led to such a violent end. She distinguishes Ceaușescu's early years in power from his later years, and certain patterns emerge. Ceaușescu's policies were, initially, in line with the hopes of both the domestic population and the international community (excluding the Soviet Union). As he consolidated power, however, Ceaușescu lost touch with his country's political reality and with popular concerns. To the international community, his foreign policy seemed to overshadow his domestic policies, which were carried to an extreme, serving foreign policy goals but ending his support base.

Ceaușescu was an astute politician. The key components of his political strategy, according to Fischer, were the celebration of Romanian nationalism on the one hand and the promise of industrialization on the other. Romanian nationalism served to justify and exalt the divergent foreign policy that Ceaușescu was able to fashion while averting a Soviet invasion similar to Hungary's in 1956. Romania remained a member

of the Warsaw Pact, but Ceaușescu defied the Soviet Union and pursued his vision of sovereignty. By asserting the independence of Romania to his fellow Romanians and to the international community, Ceaușescu deservedly won praise, according to Fischer. His emphasis on industrialization also won praise from the Romanian people and the international community. But Ceaușescu's policy soon changed from one stressing the living standards of the Romanian population to one that subordinated domestic policy to foreign policy goals.

Ceaușescu apparently recognized that an independent foreign policy required a thriving economy that could compete in world markets. Indeed, the economy prospered during the 1970s relative to other Eastern European states and accomplished some legitimacy for Ceaușescu's otherwise ideologically revolutionary government. His drive to build a self-sufficient and modern industrial economy soon translated into severe austerity measures, which permeated every aspect of life. Ceaușescu spoke highly of moral integrity, which he began to institutionalize through stringent laws abolishing divorce, abortion, and adultery—anything that might diminish the potential size of the working population. Following the same line, he stressed the ideals of the work ethic, honesty, and respect for communal or state property, all of which would ideally lead to a more efficient economy with less tolerance for laziness, corruption, and low-quality production.

Romania's economy, however, inevitably deteriorated because of its reliance on centrally planned growth. Ceaușescu began to resort to coercion and to a self-generated cult of personality, both of which became more exaggerated and malevolent as he lost genuine political support. As Fischer accurately observes, Ceaușescu needed indigenous support because he had chosen to champion national autonomy in the face of the Soviet Union.

Fischer's analysis is most successful when considering the signs and operation of Ceaușescu's bizarre personality cult. His "rags to riches" story was real, albeit not through the accumulation of wealth but through his hard work and loyalty to the orthodoxy of Marxist-Leninist ideology. Accordingly, emulating this behavior was supposed to gain advancement in Romanian society. Ceaușescu was also portrayed as a family man; what others regarded as blatant nepotism was always glorified. The elevation of Elena Ceaușescu to the position of second in command, however, was followed by the promotion of brothers, offspring, and other relatives to state or party positions, all due to his intense suspicion of the intelligentsia.

Fischer devotes an entire chapter to Ceaușescu's process of effectively promoting, circulating, and removing people from the Romanian Communist party apparatus. Ceaușescu was adept at using manipulative personnel decisions to insure that his political rivals were in obscurity and that his political

sycophants were rewarded with important responsibilities.

This manipulation led to successful control and consolidation of his personal power, yet the costs were great. The party apparatus suffered from Ceaușescu's penchant for "rotation" of cadres and his recruitment and promotion of those with "proletarian" backgrounds, often discriminating against educated specialists. To further exacerbate the problem (or to add to the personal power already amassed by Ceaușescu), he rotated personnel to many different posts throughout the country so that it was difficult for new loyalties to take shape.

Fischer critically analyzes Ceaușescu's political techniques and points out the discrepancies between early promises and Ceaușescu's performance. Before judging those promises, however, one must understand, as Fischer explains, that "participatory democracy," "consultation," "equality" were all defined in peculiar ways by Ceaușescu. His twisted definitions served his own ends. His domination, we now know, left a legacy of economic collapse, social tension, and political mistrust that may never be overcome. Ceaușescu's quarter-century will, unfortunately, not be buried easily.

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DAVID JORAVSKY. *Russian Psychology: A Critical History*. Cambridge, Mass.: Basil Blackwell. 1989. Pp. xxii, 583. \$34.95.

This is a book of fundamental value for both historians of science and Russian-Soviet society and psychologists. The book that David Joravsky has written is, in a sense, an equivalent of *War and Peace* in the literature on the history of Russian-Soviet psychology. Like Tolstoy's classic, Joravsky's book is complex. It demonstrates the intricate interdependence of psychologists (both Russian and Western), their cultural upbringing, and historical events that link their ideas with social praxis. Blended with the complex texture of historical and cultural background, individual actors enter the stage of the theater of psychology, perform their roles in the dramas, tragedies, comedies, and—at times—farces that are set on that stage, and make their exit. Some of them are described as attempting to "play for the audience," hoping to trigger immediate applause, which they may or need not receive. Others look for the approval of the powerful—to become hailed by them as the "representatives of the Truth"—but are often dismissed and even exterminated. Their intellectual heirs (who were sometimes the exterminators themselves) can be observed to construct myths about some of their predecessors and to develop selective amnesia about others. New generations of psychologists come to the stage

preset for them by the myths that become the basis for contemporary psychology's social discourse.

Joravsky's book is a refreshing alternative account of the myth-dominated historiography of Russian-Soviet psychology. By way of careful analysis, the author sets as his goal to decompose the myths that Soviet psychological officialdom and its Western counterparts (that is, the so-called mainstreams of psychology) have made up and accepted uncritically. In his role of the defiler of those "sacred" myths about Soviet psychology, Joravsky succeeds admirably. Probably no major myth about the history of Soviet psychology remains in its place, as the historical contexts and personal sides of the persons involved are displayed. In that vein, I. M. Sechenov emerges as a human being rather than a tough forerunner of Pavlovian orthodoxy; I. P. Pavlov himself is revealed as a stubborn disciplinarian of his flock who is highly ironical and resistant to the new Bolshevik order—which, decades later, made his metatheory into an obligatory ideology for psychology. And so on. History of Russian-Soviet psychology, as Joravsky shows, is a sequence of wars (real and intellectual ones) and carnivals, where participants are oftentimes dressed in one another's "theoretical clothes," attack one another in mock duels, and retire to have a drink and tell their grandchildren the "truth" invented for the given time and purpose.

Joravsky's book conveys the complexity of the development of psychology in Russia to Western readers better than any other currently available on Soviet psychology. It must be said, however, that his book is more about Russian-Soviet psychologists than psychologies. Or, perhaps more accurately, Russian and Soviet psychological ideas as presented by Joravsky through careful exposure of the characters and life events of the psychologists who invented or imported their ideas, while being immersed in their particular historical-cultural "life spaces." The book is a historian's account of the social history of Russian psychology. This means that Joravsky's book is not a substitute for the existing (or future) treatises of psychological ideas, as those are of relevance for contemporary psychology as a whole. Rather, it complements the efforts by contemporary psychologists who often have limited understanding of the ideas that have emerged in the Russian context, since they lack the intuitive "feel" for the historical context.

Last (but not least), Joravsky's book is not only about Russian psychology but also about our own. Its original title, *Russian Psychology (and Our Own)*, which was altered to include the mundane subtitle, would have been more appropriate. A major theme that seems to underlie Joravsky's work is a constant amusement about the ease with which the Soviets' myths of their psychology have made their way into the discourse of Western psychologists. In their propensity for myth creation, Western psychologists do not differ from their Soviet counterparts, and Joravsky's witty narrative reminds us about that in

multiple places. Such exposure of the similarity of "our own" psychology to that of Russia and the Soviet Union is a major accomplishment in this book. Ironically, it is exactly this similarity that guides the author—an American historian—to demonstrate his own capabilities of myth creation. The myth is really one about psychology and is built at the intersection of describing the use of psychological methods (those of syllogistic reasoning and concept formation) and of describing the Soviet social context (see pp. 364–68). The notion that what Soviet psychologists (particularly Alexander Luria) did in their experiments reflects the "authoritarian" nature of Soviet "socialist reconstruction" practices seems to project the notion of "authoritarianism" specifically onto Soviet psychology's methodology. Here another myth about Soviet psychology is being born. The same reasoning can be extended to psychology at large. In fact, every time a psychologist compels the subject to give a "forced choice" answer (including "multiple choice") to some questions, and then refuses to accept the subject's complaints that the given choice did not allow him or her to tell the "whole story," we can represent that psychologist as a brainwashed authoritarian. In this sense, all of the history of education and psychology is inherently authoritarian, and the biggest abusers of their subjects in all of psychology would be Sigmund Freud and the Educational Testing Service. In other terms, myth breaking is a treacherous undertaking, because it can lead to further myth making.

Aside from this little (psychologist's) concern with a historian's account of the methods we use, Joravsky's book constitutes a major historical data base for contemporary philosophers of science who oftentimes forget that their own discourse is a product of a historical process rather than the foundation of the ivory tower of pure science, free of social ironies and hypocrisies. Joravsky moves our understanding of Russian psychology forward by pointing out that we, self-content "Western" scientists, had better have an in-depth look into our own assumptions and activities before becoming fascinated by, or dismissive of, the curious foreign phenomenon labeled "Soviet psychology."

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BLAIR A. RUBLE. *Leningrad: Shaping a Soviet City*. (Lane Studies in Regional Government.) Berkeley and Los Angeles: University of California Press, for the Institute of Governmental Studies and the Institute of International Studies, University of California, Berkeley. 1990. Pp. xxvi, 328. \$45.00.

Beginning in 1703, Peter the Great founded and laid out a new capital on the islands and swamps at the mouth of the Neva River flowing into the Baltic. His purpose was to open and reorient the Russian empire

to the West. To make clear his intention, he brought in West European architects and craftsmen to design and build a European city. Peter's successors continued the tradition of looking to the West and built their palaces and state buildings in the grand European style. The Bolsheviks ended the monarchy and turned Russia inward. The seat of government was returned to Moscow. St. Petersburg—renamed Petrograd, which changed to Leningrad after Lenin's death in 1924—seemed destined to become just another decaying provincial relic. But the city's inhabitants did not accept this fate and fought to keep their city a major cultural, scientific, and industrial center. They succeeded. From its low point at the end of the Civil War when the population of the city was reduced to 720,000 from a pre-World War I high of 2.3 million, the first priority of the city's intellectual and Communist elites was to restore and expand its scientific community. On this foundation Leningrad built a comprehensive system of higher education, which graduated a large crop of engineers and scientists. With scientists and skilled workers at a premium as Stalin launched his Five-Year Plans of rapid industrialization, the city was ready. Leningrad soon became a major center for technological industries and high-priority defense production. By 1939 its population had grown to over 3 million, and Leningrad was recognized as the second city of the USSR. And, even though the nine-hundred-day siege of the city by German forces in World War II destroyed much of it and its industry, the fact that the population refused to surrender and continued to resist resulted in the half-starved residents being designated "heroes of the war" and showed the determination of its citizens to restore Leningrad as a major scientific, industrial, and cultural center.

This determination to be a major center was seen by Moscow and the central party bureaucrats as a threat, and periodically the Leningrad party and government elites were purged to assure Moscow's continued supremacy. But Moscow was unable to purge or weaken Leningrad's eminence as a scientific center. It was too important to the security and economic development of the country. Furthermore, Moscow could not find the basis to challenge Leningrad's scientific community on ideological grounds. Contrary to what one might expect—that Leningrad would be a center of liberal opposition with its traditional orientation to the West—the leaders of Leningrad's scientific community were staunch supporters of orthodox communism and leading proponents of planned socialism.

Beginning particularly with the General Plan of 1936, the development of Leningrad and the surrounding territory has been guided both in theory and in practice by that plan. When the plan proved to be inadequate or when the conditions and needs changed, the plan was revised. By the 1960s, Leningrad had become the center for city planning in the

USSR; its scientific institutes drafted the general plans for several cities in the Soviet Union.

Blair A. Ruble, director of the Kennan Institute for Advanced Russian Studies, traces in detail how city planners and party and city government leaders guided Leningrad's development by means of the General Plan. Although other cities and regions in the USSR have had general plans, they have had a minimal influence on actual development. The Leningrad plan not only guided development but was also supported by the party and government and economic elites. What is even more surprising was Leningrad's ability to get from the central planners the resources necessary to carry out its General Plan. How did Leningrad manage to succeed when almost all other cities and communities failed?

Unfortunately, Ruble's detailed chronology of city planning and development of Leningrad does not explain in full its success. The published record, which is his primary source, provides less than half of the story. The politics and personal relations of the elites within the city and with the central planners were never discussed in public. Perhaps with the coming of an open democracy to Leningrad in 1989, it may be possible through interviews and the opening of archives to find the politics of socialist planning as it operated in Leningrad. Although Ruble does not ignore completely the politics of socialist planning, he does not warn the reader that the key variable in explaining its success may be not the sophisticated planning of Leningrad's elites but politics and personal relations that he was unable to investigate. Nevertheless, this study of the technical aspects of socialist planning and the role of the various scientific institutes in drafting the General Plan is useful in providing some balanced analysis at a time when all socialist planning is being condemned as having failed.

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ANTHONY L. H. RHINELANDER. *Prince Michael Vorontsov: Viceroy to the Tsar*. Montreal: McGill-Queen's University Press. 1990. Pp. viii, 279. \$34.95.

Biographies of Russian statesmen and administrators are rare, and biographies of those who devoted themselves to governing the empire's vast hinterlands are rarer still. Therefore, Anthony L. H. Rhineland-er's study of Michael S. Vorontsov is doubly welcome. Between 1823 and 1855, Vorontsov ruled the enormous area known as New Russia, those territories along the northern shore of the Black Sea that had been acquired, for the most part, during the reign of Catherine II. From 1844 until his retirement, he also served as the tsar's viceroy in the Caucasus. Thus, a work on Vorontsov promises much: the life story of a fascinating individual, insights into the problems of

Russia's bureaucracy at the local level, and an understanding of the processes of empire building and imperial administration. To a large extent, Rhineland's well-researched and readable book fulfills its promise.

The author does a fine job of tracing Vorontsov's public and private career. He portrays Vorontsov as a builder and a modernizer who attached the greatest importance to stimulating the economic and cultural development of the regions entrusted to his care. Although Rhineland gives considerable attention to his subject's administration of New Russia, he is at his best when he recounts Vorontsov's struggle to put Russian rule in the Caucasus on a firm footing. The problems there were especially daunting. The region was distant and backward, and its population had long been fragmented along ethnic, religious, and administrative lines. The ancient civilizations and cultures of the region could not be easily russified. More serious still, the tsar's rule was threatened by Muslim guerrillas who, under the leadership of the legendary Shamil, were waging a protracted struggle for freedom.

Rhineland credits Vorontsov with considerable success in coping with these difficulties. Unable to suppress Shamil and his followers by means of a military campaign, Vorontsov quarantined the rebellion and fatally weakened it. In addition, the viceroy reorganized local administrative units to take better account of ethnic distinctions and historic divisions. He paid great attention to indigenous traditions, fostering the growth of education, literature, and journalism in the native languages. Simultaneously, Vorontsov drew Caucasian elites into the tsar's service, providing avenues for training and advancement. Yet Rhineland shows that Vorontsov's achievements had paradoxical consequences. Initially, they strengthened Russian control, but, by helping revive Georgian, Armenian, and Azerbaijani culture, Vorontsov's measures ultimately fostered the local nationalisms that were to threaten both imperial and Soviet rule.

Rhineland focuses considerable attention on Vorontsov's administrative style. He shows how Vorontsov, as governor-general and viceroy, built up a strong cadre of energetic, competent officials who enjoyed his respect and solicitude. Vorontsov drove his subordinates hard but did not spare himself, for he was always actively engaged in many of the details of governance. He also used his close ties to the emperor in order to cut through red tape and avoid many of the constricting regulations that emanated from the St. Petersburg bureaucracy. Although Rhineland provides an excellent picture of Vorontsov practicing the arts of government, he might have strengthened this aspect of his work by placing his subject more firmly in the political and administrative context of his age, showing clearly how Vorontsov's style differed from that of other leading provincial officials in the reigns of Alexander I and

Nicholas I. But perhaps this is too much to ask of a biography. We should, instead, be grateful to Rhineland for this lucid account of Vorontsov's life and for the insights he gives us about the problems and character of imperial rule in the first half of the nineteenth century.

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A. A. LEVANDOVSKII. *T. N. Granovskii v russkom obshchestvennom dvizhenii* [T. N. Granovskii in the Russian Social Movement]. Moscow: Izdatel'stvo Moskovskogo universiteta. 1989. Pp. 251. 2 r. 20 k.

Timofei Granovskii was one of the central figures in Russian intellectual life of the 1840s, exerting influence both from the university lectern and in Moscow salons. He was an extremely popular and influential history professor at Moscow University, one in the phalanx of young scholars who brought Hegel's philosophy to their own interpretation of their subjects, and he was one of the leading members of the Westernizer circle.

But despite Granovskii's importance to his contemporaries, he remains without a full-scale biography. Moreover, given the paucity of sources, a satisfactory study may never be written. Not only is information on Granovskii's personal life scanty but much of his public life was conducted orally through university lectures and, even more important, through conversations with friends and opponents. He would have liked to publish his views, but in the Russia of Nicholas I this was never possible. Therefore, student notes and reminiscences of friends constitute the main sources for our understanding of his opinions.

Andrei A. Levandovskii's study provides an account and interpretation of Granovskii's intellectual biography. It makes no claim to be a full-scale biography; Granovskii's personal life disappears completely once his childhood has been covered. The impact of personal events, even those that Granovskii acknowledged had had a profound effect on him, such as the deaths of Nikolai Stankevich and of his own sisters, is ignored. His vices do not rate a mention. Granovskii's personality is discussed, but only as part of his interaction with other male intellectuals. Moreover, the research base for the book is very narrow; with a few exceptions, all of the cited sources are published nineteenth-century primary and memoir literature. Non-Russian studies of Granovskii and the circles are ignored. Thus, little in the book will be new to a specialist. Lacking sufficient information on his subject, the author devotes a disproportionate amount of attention to Granovskii's milieu, sometimes leaving Granovskii himself in shadow and out of focus.

Despite these limitations, the book is intelligent, interesting, and useful. Levandovskii views Granovskii primarily as a public figure (*obshchestvennyi deiatel'*)

rather than an academic. Thus, he does not believe that the absence of a large body of historical work is a serious failure: Granovskii was an educator, an enlightener, and a propagandist for whom the academic side of his profession was distinctly secondary. The chapter on the university is particularly good, with a discussion of the main players in university factions and an analysis of Granovskii's historical views. Although the basic narrative on the circles is drawn primarily from Alexander Herzen and thus is familiar, Levandovskii's interpretation of Granovskii's position vis-à-vis Herzen is quite interesting. Also of substantial value is the account of the historian's last years and the uses made of Granovskii's name and reputation by various factions after his death.

Levandovskii has provided a useful summary of Granovskii's life for students as well as interpretations that historians of the period should take into account.

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FRANCIS WILLIAM WCISLO. *Reforming Rural Russia: State, Local Society, and National Politics, 1855–1914*. (Studies of the Harriman Institute.) Princeton: Princeton University Press. 1990. Pp. xviii, 347. \$45.00.

This book analyzes one of the most significant problems in imperial Russian history, that is, tsarism's failure to adapt its rural institutions to social and economic change from the peasant emancipation of 1861 to 1914. Using the politics of rural reform as a vehicle to illuminate the *mentalités* of elite officials, predominantly in St. Petersburg, Francis William Wcislo argues that Russia failed to evolve from an autocracy that ruled a society of segregated estates (*soslov'ia*) to a modern nation-state that governed a civil society. The reason in his view was tsarism's dependence on the estates order. Time and again official proponents of rural administrative change were defeated by ministers and nobles who defended state tutelage and noble privilege in the countryside.

In separate chapters Wcislo analyzes the politics of the peasant emancipation, the autocratic crisis (1878–82) and the Kakhanov Commission's plan to overhaul local government, the rural counterreforms of the 1880s, the development of national politics, Sergei Witte's ministry and his reform plans before and during the 1905 revolution, government and politics under Peter Stolypin (1906–07), and the provincial nobility's role in thwarting Stolypin's plan for rural administrative reform—a defeat that underscored the government's isolation by 1914. Throughout his study, Wcislo maintains that the reform debates matched the generalists, who advocated the development of legal, all-estate administration (*veseslovnost'*) in rural Russia, against the traditionalists, who defended police power, separate estates, and the use of

noble landowners as state agents to supervise peasant administration. Ironically, as Wcislo shows, the defenders of separate *soslov'ia* proved increasingly adept at preserving a rural structure that became more anachronistic each year. Yet equally important, after 1905 even reformers such as Stolypin used dictatorial methods in policy making that subverted their aim to create a new civil order and discredited them with elite society.

Unrivalled in its broad chronological sweep on the topic, Wcislo's book tells us much about the estates issue in debates over rural reform, especially for the core of his study covering 1894–1914. Here he uses much new material from newspapers, archives, and state commissions to delineate the course of and public reaction to the debates. His chapter on the development of Witte's ideology of rural administrative reform is particularly good. Although Iu. B. Solov'ev, Neil Weissman, and Roberta Manning, among others, have covered similar ground, Wcislo provides the most thorough and lucid account of rural reform politics under Nicholas II. He shows that the old regime missed several chances to establish a mediating link between the state and village because, many officials believed, peasants were not yet prepared to participate as equals in civil society.

Nevertheless, not all historians will agree that the estates question was the overriding issue in the politics of rural reform. Indeed, the book's principal limitation is that, despite its title, it provides little concrete information on the shortcomings of rural government, the extent of rural change, or provincial aspirations to reform rural Russia (especially for the pre-1905 era). Although Wcislo's purpose is not to focus on rural administration itself, the reader nonetheless needs more context to assess the validity of arguments made by the generalists and their foes. Similarly, Wcislo's evidence suggests that bureaucratic control was perhaps more significant than the estates issue in generating the diverse opposition (ministers, nobles) to Stolypin's proposal for an all-estate *zemstvo volost'*. The United Nobility's opposition not only expressed fear that the landed nobility would disappear in such an institution but also attested to years of noble resentment over bureaucratic interference in local self-government. These points notwithstanding, this book is a most welcome addition to recent scholarship on the end of the tsarist regime.

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LEONAS SABALIŪNAS. *Lithuanian Social Democracy in Perspective, 1893–1914*. Durham, N.C.: Duke University Press, with the cooperation of the Association for the Advancement of Baltic Studies. 1990. Pp. viii, 205. \$29.95.

Leonas Sabaliūnas's slim volume on the Lithuanian Social Democratic party (LSDP) in the two decades

before World War I is a useful if modest addition to the existing historical literature, especially in view of the paucity of English-language sources on this period in Lithuanian history. Despite the overwhelmingly rural character of the regions inhabited by the Lithuanians, it is striking that the first specifically Lithuanian political party to emerge was the Social Democratic party in 1896. This event is best explained by Lithuania's close ties with Poland, where the Polish Socialist party (PPS) had already been founded by 1892. As Sabaliūnas correctly notes, a key factor to bear in mind when assessing Lithuanian social democracy is the intricate multinational context in which it operated. The fortunes of the LSDP closely paralleled those of the revolutionary movement in the Russian empire as a whole. Following successful government repression in the late 1890s, a resurgence began and culminated in the Russian revolution of 1905. Nevertheless, Lithuania lagged behind more activist regions of the empire in that year, and the LSDP failed to capture a leadership role at the most representative Lithuanian assembly of 1905—the Grand Diet of Vilnius. The party's most tangible success was the election of five of seven Lithuanian deputies to the Second Duma. After 1907 the party's fortunes declined, reaching a low point in 1909–12; however, the empire-wide strike movement in 1912 fostered a new revitalization.

The author focuses mainly on ideology and argues that the LSDP's program was largely shaped by westward contacts, above all the PPS and the German Social Democrats. This orientation encouraged a critical view of Russia's overall development and skepticism about its potential support for the Lithuanian revolutionary movement. According to Sabaliūnas, the national question dominated LSDP politics and overshadowed all other issues, including social questions. The strength of the Polish connection, coupled with a fear of Russian domination, led the Lithuanian party to call for a federation with Poland, Latvia, Belorussia, and the Ukraine—but not ethnic Russia—at its first congress in 1896. Despite some dissent within the LSDP, this position remained unchanged until 1905. In the wake of the revolution, the Lithuanian Social Democrats moved more toward the goal of autonomy in a transformed Russian state, mainly as a means of smoothing the way to alliances with other leftist parties in the region. This shift in orientation, however, bore little fruit because only the local affiliate of the PPS in Lithuania was willing to establish formal ties with LSDP. The Bund, the internationalist Polish Social Democrats (SDKPiL), and the Russian Social Democratic party all regarded the LSDP as overly nationalist.

Although this book provides considerable information on the issues with which the Lithuanian Social Democrats grappled, there are a number of missed opportunities. Although the author makes good use of the LSDP press, the focus on internal party debates is overly narrow. We learn little about the LSDP's

social and territorial base or about the dynamics of competition with other Social Democratic parties. The context of the study remains vague with no attention paid to ethnic composition. How, for example, did the tiny Lithuanian presence in Vilnius (2 percent in 1897) affect the party's fortunes? The book could also have used some rigorous editing to avoid lapses into a turgid style, and, given the administrative fragmentation of the Lithuanian areas under tsarist rule, a good map would have been helpful.

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JOHN F. HUTCHINSON. *Politics and Public Health in Revolutionary Russia, 1890–1918*. (The Henry E. Sigerist Series in the History of Medicine.) Baltimore: Johns Hopkins University Press. 1990. Pp. xx, 253. \$42.00.

John F. Hutchinson has written a solid monograph on the politics of health in early twentieth-century Russia. He carefully examines a history of conflict and accommodation between the state and medical profession over the control of public health during the last decade of the tsarist regime, the revolutions of 1917, and the first months of Soviet power. In a well-written and nuanced treatment, Hutchinson seeks to dispel conventional (or once conventional) mythology about an irredeemably obscurantist tsarist bureaucracy, as well as the more enduring myth of progressive zemstvo professionals.

Despite its title, the heart of this book deals with the period after 1905. A background chapter describes the principal actors: medical bureaucrats who fought to replace the overlapping jurisdictions that stymied reform with a coordinated national health policy under a central ministry; and the community physicians working for zemstvo and municipal local governments who had done so much to advance popular health in recent decades, who advocated a politically informed practice of medicine, and who doggedly resisted any state effort to circumscribe local autonomy. The second chapter deals with the radicalization of this populist-oriented profession during 1905. The author covers familiar ground (and repeats one error, that twenty-five thousand physicians joined the Medical Union of 1905; the figure is more like fifteen hundred).

It is in his analysis of the disarray of community medicine after the revolution that the author breaks new ground. The "crisis" of zemstvo medicine resulted not just from the political context of zemstvo reaction and failure of Stolypin's local government proposals but from widening divisions within the medical community caused by scientific developments in bacteriology, epidemiology, and sanitary engineering (water supply) as well as from greater technical specialization that threatened the traditional ethos of

the socially active zemstvo generalist. The same European-wide trends, Hutchinson points out, also reinforced arguments for greater centralization of public health, if only to maximize research and implement national anti-epidemic measures. Government reformers such as G. E. Rein, who insisted that cholera undermined popular faith in the regime, argued along these lines. Ironically, it was the Ministry of Interior under P. A. Stolypin, who had similar motives, that tried to thwart Rein's plans.

World War I transformed the politics of health. Pioneering bacteriologists and other specialists enconced themselves in the Unions of Zemstvos and Towns where, because of the government's inability to handle the colossal problems of wounded, refugees, and epidemics, they were able to achieve unprecedented influence and room to maneuver. The war allowed these young reformers to implement programs and plan for the postwar order; it convinced many of them of the need for a centralized health agency, if not necessarily Rein's ministry. The February revolution removed the latter qualm, but the Provisional Government's paralysis and the deepening social polarization of the summer of 1917 convinced a tiny minority of physicians such as Z. P. Solov'ev to support the Bolshevik seizure of power. After several months of confrontation, the new regime demonstrated its moderation by upholding the professional expertise of physicians in the face of egalitarian challenges from army feldshers and by setting up a commissariat pledged to tackle mounting health crises. Consequently, community physicians made their peace with the new order.

Hutchinson's heroes are establishment reformers such as Rein and the wartime union innovators who, although not allies, both confronted the resistance of conservative bureaucrats and the deep-seated prejudices of "traditional" community physicians. There are limits to this interpretation. The book focuses nearly exclusively on the leadership of the medical profession (for example, the Pirogov Society) and the St. Petersburg bureaucracy. This is not a book about physicians as a group, their attitudes, or their experiences in local communities. Still, from the scant information Hutchinson provides about the rank and file, the concerns raised by *pirogovtsy* such as S. N. Igumnov and D. N. Zhbakov over a decline in idealism and *obshchestvennost'* among the post-1905 generation of physicians seem real, and these trends may have contributed to growing social polarization between intelligentsia and masses in the last decade of imperial Russia. Along the same lines it seems that their suspicion of the new professional model of the physician as a technical specialist, whose expertise would benefit the population despite little contact with it, reveals more than a knee-jerk populist prejudice, as Hutchinson would have us believe. And finally, community physicians' wariness regarding a tsarist health ministry makes a good deal of sense in light of the history of zemstvo-state conflict in this

field and the fact that Rein's reforms would have severely undercut zemstvo medicine. Perhaps in their sensitivity to the broader social context of public health and in their estimation of the regime's capacity to reform, Igumnov and Zhbakov were not quite the dinosaurs Hutchinson describes. A more satisfactory judgment on this score would require much closer examination of physicians' activities in the provinces during the post-1905 era, which is not the focus of this book. As it is, we get only a vague sense of rank-and-file opinion on the issues discussed here and how that opinion might have shaped the debates at national medical congresses and in the press.

Hutchinson's study is based on a wide variety of these and other published sources but not archives that might have added a useful dimension to his treatment of the health ministry issue. All in all, he offers a valuable perspective on the political history of the period between the revolutions, a pivotal era that surprisingly has received relatively scant attention.

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TEODOR SHANIN. *The Roots of Otherness: Russia's Turn of Century*. Volume 1, *Russia as a "Developing Society"*; volume 2, *Russia, 1905–07: Revolution as a Moment of Truth*. New Haven: Yale University Press. 1986. Pp. xvi, 268; xvi, 379. Cloth \$60.00 the set, paper \$19.90 the set.

Teodor Shanin's *The Awkward Class* (1972) was an awkward classic—a significant and challenging argument often concealed in thickets of impenetrable prose. The present study, in some measure an elaboration of his earlier book, shares both the strengths and the weaknesses of its predecessor.

This important, insightful work is at once a treatise on the capitalist world economy, a sociological analysis of peasant behavior, and a history of rural unrest in Russia (1905–07). At heart, however, this is a study of cognition and the sociology of knowledge. Shanin argues that Russian peasant society has been consistently misunderstood—by reformers, revolutionaries, and policy makers of tsarist and Soviet times and by the historians, economists, and sociologists who have offered ex post facto explanations for Russian developments.

Almost all observers, he suggests, tried to squeeze Russia into the Procrustean bed of European experience. This was particularly true before 1905, when liberals and socialists alike were enamored of unilinear models of historical progress. The experience of England, France, and even Germany suggested that the small-holding peasant was a historic anachronism hopelessly doomed by the advance of capitalism. To Russian Social Democrats, the term "peasant revolution" was almost an oxymoron; peasants would support the cause of socialism only if, as Friedrich Engels

said of German peasants, "we make them a promise which we ourselves know we cannot keep" (vol. 2, p. 174).

And yet in 1905–07 the Russian empire was swept by a peasant jacquerie that confounded all expectations. In Shanin's account, this was a spontaneous upheaval with its own revolutionary agenda, almost independent of unrest in the cities or factories. Not even Russia's agrarian socialists (the Socialist Revolutionary party) managed to exert much influence on the course of rural upheaval, yet peasant disorders continued for almost two full years after the urban revolution of 1905 had been crushed. This was the "moment of truth" of Shanin's title. He spends much of volume 1 explaining why such a movement occurred and volume 2 outlining how various historical actors assimilated the lessons of those turbulent years.

Chapters 4 and 5 of volume 1 set forth the author's theory of backwardness. Was Russia a "late developing" capitalist state like Bismarck's Germany or, like China, a peripheral dependency of a worldwide capitalist system? Despite the country's impressive industrial growth, Shanin favors the world-systems view. He argues that Russia embodies many characteristics of the "dependent development" model expounded by (among others) A. G. Frank and Immanuel Wallerstein: "growing international debt and linked financial and technological dependence endangered long-term 'growth.' . . . State-supported industrialization facilitated severe crises of agriculture and of rural society increasingly treated as a milking cow and a dumping place of 'modernisation' and 'growth' focused elsewhere" (vol. 1, p. 199). These forces collided with the specific features of the Russian countryside, making peasant agriculture "different in structure, functioning, logic of response and patterns of change from the 'model' [of] classical capitalism" (vol. 1, p. 172). The net result was a situation of unique revolutionary potential. The outburst of 1905–07 "was part and harbinger of a new wave of rising peasant identity . . . perpetrated not by the uprooted but by those who were refusing to be turned into such" (vol. 2, p. 172).

In the revolt itself, the author emphasizes the peasants' autonomy, coherence, and self-restraint. Unrest was organized around preexisting structures of communal existence but also found expression in the short-lived All-Russian Peasant Union, whose nonviolent tactics are described as best representing the views of mainstream peasant activists. Some of Shanin's most impressive pages are devoted to the "Peasant Dream," a synthesis of old and new: "[a] terminology of conservatism, conventionality, patriarchalism, religion and often parochialism and semi-magical beliefs injected with new and radical words, views and experiences, and put to use to grasp and shape a rapidly transforming society" (vol. 2, p. 135).

Chapters 5 and 6 of volume 2 pursue the theme of reconceptualization by tracing the reactions of various parties and individuals to the peasant upsurge.

Shanin concentrates on four prominent figures who tried to answer the agrarian challenge: Peter Stolypin, Leon Trotsky, V. I. Lenin, and the Georgian Menshevik Noi Zhordaniia. Their responses ranged from Stolypin's "wager on the strong" to Lenin's "democratic dictatorship of workers and peasants," but all four shared a willingness to revise their former views in the face of intractable reality. The solutions that they proposed proved only partially viable. Stolypin's "reform from above" did not prevent the renewal of peasant revolution in 1917; Lenin and his allies rose to power on a tide of peasant militancy but failed to solve the greater problem of peacefully incorporating peasants into a socialist polity.

Like his protagonists, Shanin tries to see the world through a new lens. He offers the reader an argument, not a monographic exposition. His evidence, often impressionistic, will be familiar to most historians, but his synthesis is novel and provocative. His eloquent evocation of the peasant world and passionate denunciation of historical-sociological dogmatism give the work a broader relevance to studies outside the Russian field. The portraits of Lenin and Stolypin raise more questions than they answer, thereby reinforcing the message that real life is more complex and contradictory than ideological formulas.

For all of its positive qualities, however, the book leaves me feeling frustrated, even exasperated. Reviewers are normally advised to stick to matters of substance, but the form of the present work presents exceptional problems. Its editors have simply not done their job. Shanin's disjointed, elliptical exposition does not do justice to the author's originality. One wonders for what audience the book was written; it ranges from textbook-like narrative to densely theoretical discussion. The numerous stylistic and typographical mistakes sometimes have a certain charm (Lenin "challenging the 'holy cows' of his lieutenants" [vol. 1, p. 157]) but are more often puzzling or misleading ("everness" for "awareness" [vol. 2, p. 308]; "conjectures" for Fernand Braudel's "conjunctures" [vol. 2, p. 314]). Many readers will lack the patience or imagination to discern the butterfly in this uncomely chrysalis.

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LYNN MALLY. *Culture of the Future: The Proletkult Movement in Revolutionary Russia*. (Studies on the History of Society and Culture.) Berkeley and Los Angeles: University of California Press. 1990. Pp. xxix, 306. \$37.50.

Proletkult, a radical proletarian cultural organization, was founded just days before the October revolution. Inspired by the world's first worker government, adherents pursued new forms of proletarian art,

culture, and science that they thought essential to a worker society. Proletkult flourished during the Civil War (1917–20). Hundreds of thousands of young people joined branches in the big cities and provinces where they studied theater, poetry, choral music, and art. Proletkult gained renown by defending the purity of proletarian culture and asserting its autonomy. Although independent from the state, the organization received generous state funding. This led to a famous confrontation in late 1920 when Proletkult was subordinated to the state Committee for Political Education and suffered severe budget cuts. Soon there were only several units in the biggest cities and a few in provincial centers. When Soviet cultural organizations were consolidated in 1932, Proletkult ceased to exist.

Lynn Mally's fine book traces the history of this organization from its prerevolutionary advent to its demise in 1932. Unlike most authors on the subject, Mally does not limit discussion to the ideological enmity of Vladimir Lenin and Alexander Bogdanov or the rhetoric and activities of Proletkult's Moscow leadership. Although Mally pays these questions scrupulous attention, she looks more closely at the rank and file and draws an intriguing picture of cultural politics amid revolutionary change. Mally uses archival documents to portray the beliefs and ambitions of Proletkult's young members. The Proletkult program was promulgated from the center and articulated in a class-based language notable more for its instrumentality than for its clarity. Beneath the rhetoric, Proletkult showed contradictions that cut to the heart of the revolution. The young proletariat portrayed by Mally was unsure of its identity and future yet emboldened by the revolution to experiment. Leaders and members were divided by rank, geography, and generation. These differences, disregarded by Proletkult policy, had a profound impact on its practice: young club members often ignored or rejected the tutelage of the older, more educated presidium members in Moscow; communications between the center and the provinces were tenuous; and local clubs were often not even aware of what the central leaders considered fundamental principles. Documents reveal conflicts over the very nature of the working class. Whereas radical membership regulations demanded strict worker credentials, local groups achieved mass enrollment by accepting peasants and middle-class sympathizers, and though older intellectuals were branded class enemies, clubs employed them as the best available teachers. Class identity plagued even the talented worker-students slated by Proletkult to lead the future society. They needed full-time study to attain their potential, but full-time study displaced them from the factories that made them workers in the first place. Perhaps the deepest controversy involved the "culture of the future" itself. Some Proletkultists rejected the classical legacy and wrote paeans to factory life; others preferred an avant-garde style much maligned by proletarian purists; yet others

preferred the example of prerevolutionary, nonproletarian masters.

Mally has used a wealth of new materials to portray the rich and contradictory culture of the Russian revolution. Inspired by an opportunity to change the world, young Proletkultists spent their leisure time studying and creating art. Buffeted by political battles and torn by conflicting visions of the future, their activity evoked many reactions and left a mixed legacy. Some of the darkest campaigns of Soviet cultural history employed tools forged by proletarian culture movements, yet Proletkult also convinced a generation of young workers of its right to cultural expression.

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R. CRAIG NATION. *War on War: Lenin, the Zimmerwald Left, and the Origins of Communist Internationalism*. Durham, N.C.: Duke University Press. 1990. Pp. xviii, 313. \$45.00.

If the term "overkill" may be properly applied to historiographic concerns, surely the origins of modern communism would easily qualify as a case in point. From Merle Fainsod's *Socialism and the World War* (1935), a pioneering classic, to David Kirby's impressive *War, Peace and Revolution* (1986), a veritable deluge of memoirs, articles, monographs, and biographies has descended on the academic community with a substantial spillover to the larger reading public. This occurrence obviously derives less from the pursuit of arcane scholarship for its own sake than from the political and ideological repercussions of the Bolshevik revolution. Who but connoisseurs of radical Marxism and its vitriolic disputes would recall the Zimmerwald conference, or even Vladimir Lenin, if tsarist Russia had survived World War I with Nicholas II firmly on the throne?

R. Craig Nation's book is the latest effort to survey the debacle of the Second International and the birthing rites of the third or Communist International. Readers might logically inquire why and how this apparent exercise in redundancy found its way into print. Anticipating the question, the author provides an answer that is not wholly convincing but serves in part to legitimize his labors: previous scholars have concentrated on wartime socialism in general terms, whereas his investigation focuses on the Zimmerwald Left. Soviet historians, zealous chroniclers of Lenin's every thought and deed, have been guilty of "self-defeating idealizations and politically motivated distortions" (p. xi), obviously a polite understatement as applied to the Stalin era. Despite the astonishing developments under Mikhail Gorbachev, "unspoken taboos remain in place" (p. xi). One wonders what they could be, for even Lenin is no longer a secular icon, and Leon Trotsky is receiving his just

due, although he has not yet been officially rehabilitated.

The core of the volume does probe more exhaustively and, arguably, more cogently than rival studies of the Zimmerwald conference and its aftermath. Reference notes and an extensive bibliography in the major European languages, including an array of archival sources (but excluding those of the Soviet Union), furnish ample documentation. The narrative falters on occasion amid the arid tedium of socialist exegesis, but, for such an intellectually demanding work, the pace is brisk and the prose lucid and relatively jargon-free (I noted only a few "parameters" and "paradigms," the current "buzz words" of social science).

Ironically, Nation partially "rehabilitates" Lenin (and certain aspects of Leninism) at a time of self-congratulatory remarks by Western commentators on the verities of capitalism and the collapse of communism. He appears to accept, at least implicitly, Lenin's strictures on the fraudulence of bourgeois democracy and the predatory nature of capitalist imperialism, although it is not always clear when he is expressing his own views or simply paraphrasing Lenin's. He is, however, no mere apologist for the Bolshevik leader. In devoting more than perfunctory attention to Lenin's *State and Revolution*, for example, Nation explicitly analyzes three "conceptual errors" (p. 195) and demonstrates no reluctance elsewhere to differ with Leninist doctrine. Soviet history as such, including Lenin's brief tenure as "dictator," lies outside the author's major theme. But Stalin makes more than a token appearance, and there is a somewhat strained attempt to link the Zimmerwald legacy to contemporary events.

This book is not the last word on the subject, but its authority (and that of its predecessors) is such that scholars would be well advised to declare a moratorium and proceed to less crowded fields. Nearly fifty years after its demise, the Communist International has yet to find its historian. Perhaps Nation will volunteer his services.

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MARK VON HAGEN. *Soldiers in the Proletarian Dictatorship: The Red Army and the Soviet Socialist State, 1917–1930*. (Studies in Soviet History and Society; Studies of the Harriman Institute.) Ithaca: Cornell University Press. 1990. Pp. xviii, 369. \$29.95.

Mark Von Hagen's major argument is that the Red Army helped bring on Stalinism in three crucial ways. First, during the Civil War, a professional military organization and ethos triumphed over democratic and egalitarian principles; the triumphant Civil War army provided a compelling metaphor and a model for the construction of an authoritarian state, or what Von Hagen calls militarized socialism. Second, mili-

tary leaders and the army's political staff formed for political and professional reasons an important constituency inclined against the New Economic Policy (NEP) and toward the support of Stalin. Finally, service in the army turned many peasant soldiers into ardent proponents of the transformation of rural life along military socialist lines.

Von Hagen does not insist that the connections to Stalinism were straightforward. For instance, he argues that the Civil War was only the first of two crises that shaped the army as a professional institution. The other was collectivization. Military leaders—after a decade of attributing an important role to the army as promoter of government social policies—came to fear that collectivization would destroy troop morale. In early 1930, they led the resistance to crash collectivization, then ostentatiously declared that soldiers and officers could not spare time from their military training for civilian campaigns. Von Hagen suggests that the epilogue to this story was the purge of the generals, but there is no particular reason to suppose that was so. Moreover, his evidence for military resistance to collectivization amounts to no more than the gossip that circulated among diplomats. Von Hagen does document military withdrawal from civilian programs by late 1930, but his discussion of what he considers a crucial episode in the history of military-civil relations is curiously perfunctory.

Whatever army commanders may have done in early 1930, from 1919 on they had to contend with the fact that the overwhelming majority of their troops were peasants. They generally favored policies aimed at winning peasant support for the Soviet government: the government provided special privileges for soldiers' families, and the army's Political Administration worked tirelessly to win peasant soldiers to the regime's goals. Political officers, Von Hagen shows, were far more likely than line officers to insist on the need to preach the proletarian gospel and transform—depeasantize—the peasantry; they emerge from this study as the core of future Stalinists. Junior political officers' attitudes especially were shaped by the Civil War army, and they viewed any deviation from the Civil War pattern as a betrayal of the revolution.

The impact that army political education made on soldiers is perhaps harder to pin down than Von Hagen believes, despite his fascinating review of the evidence. Questionnaires distributed to soldiers before and after military service, soldiers' letters to newspapers, and the occasional revealing report by political officers leave no doubt that some soldiers enthusiastically took up the call to transform the village. Yet at critical moments the bulk of the soldiers seem to have sided with the peasants against the regime. Von Hagen too often blurs the difference between the soldier activists and the mass of soldiery. (Jeffrey Brooks, working with similar material from the peasant press of the 1920s, provides a model for distinguishing between peasant activists and the resis-

tant majority.) Nevertheless, as Von Hagen properly notes, the fact that any body of soldiers found the Red Army a model, or saw in it a path for social mobility, or attended to political instruction, was a major achievement that had utterly eluded the tsarist army. And even as a minority, the soldier activists helped carry out collectivization in their villages.

Occasional confusion about the extent of soldier activism aside, Von Hagen provides a pioneering and nuanced account of the Red Army as a social institution. That allows him to add depth to the familiar proposition that the Civil War was the formative political experience for the Soviet Union. And in demonstrating how a Stalinist constituency emerged at the top and bottom of the army during the 1920s, Von Hagen complements the work of Sheila Fitzpatrick and Lynne Viola, among others. This is a fine study that casts genuinely new light on the crucial early years of Soviet society.

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IU. P. BOKAREV. *Sotsialisticheskaia promyshlennost' i melkoe krest'ianskoe khoziaistvo v SSSR v 20-e gody: Istochniki, metody issledovaniia, etapy vzaimootnoshenii* [Socialist Industry and Small-Scale Peasant Farming in the USSR in the 1920s: Sources, Methods of Research, Stages of Interrelations]. Moscow: Nauka. 1989. Pp. 310. 4 r. 20 k.

This monograph is a contribution to the huge literature that, since the 1920s, has fueled the often acrimonious debate over the structure and operation of the Soviet national economy. In common with many of its predecessors, this study focuses on the respective roles of workers, peasants, institutions of the state, and the Communist party in the development of the Russian or Soviet economy after the revolutions of 1917. After lurching through the economically ruinous programs of expropriation and worker control known as War Communism, Bolshevik policy in 1921 turned toward limited privatization and restoration of domestic markets. The new program, known as the New Economic Policy, set the stage for a sharp debate during the remainder of the decade among both politicians and economists over the future of economic policy in the first Communist state. Iu. P. Bokarev's book is not the first work of this genre to have been executed by a highly competent Soviet econometrician, but it is unquestionably one of the best.

In view of the debates currently raging in Moscow and the capitals of several Soviet union republics, this work is especially timely. In spite of the author's exceptional qualifications as a historian and econometrician, however, the study bears the earmarks of a tradition of Soviet scholarship that tends to cast historical problems into a debate whose terms are structured by Leninist and Bolshevik traditions and

values—in particular, dialectical and class conflict. As such, the book reflects some of the same problems that vex and becloud current debates about the immediate economic future of the USSR.

Mathematically and statistically, the work is both interesting and sophisticated but, I think, too technical for all but a tiny minority of Western readers—those who understand multivariate analysis and Russian and who are interested in this important moment in Russian history or in historical analogues to current policy debates in Moscow. I realize that such a cohort of scholars exists, but I think the broader profession might also be usefully served by a work that addresses the same topics but confines at least some of the more technical considerations to appendices.

Time series multivariate models are central to the author's theoretical presentation of market alternatives. The formation of these models is, of course, closely linked to mathematical and statistical portions of the analysis, but I suggest that they could have possibly been presented in a less technical way that would have engaged more readers. Given that they attempt to integrate a classical understanding of the interaction of factors that either clear or fail to clear the market with a Marxist conception of the value of labor, these models are highly interesting in spite of their complexity.

In view of its serious scholarly intent, this book should have provided its readers with more of the helpful bibliographic tools of scholarship. For example, comprehensive lists of tables, graphs, and diagrams should be included in a work that relies so heavily on such materials. A bibliography, or at least a list of works cited, would be helpful to scholars who have more than a casual interest in this subject. An index would also be useful.

Bokarev's study is an impressive achievement in several respects. Even though the terms of analysis are cast in a Marxist-Leninist framework that ultimately draws too much attention to class conflict, the work nevertheless maintains a balance of viewpoint that would have been unlikely in this genre even a short time ago. It has assembled and offered for evaluation interesting arrays of data. Although they are likely to prove discouraging to many readers, perhaps the most impressive achievements of the study are its mathematical-statistical analyses and the historical evaluations of market relations based on these models. In all likelihood, these analyses used software and computational machinery that are far more cumbersome to employ than those commonly available in the West, attesting to the exceptional technical and scholarly competence of the author.

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R. W. DAVIES. *The Industrialisation of Soviet Russia. Volume 3, The Soviet Economy in Turmoil, 1929–1930.*

Cambridge: Harvard University Press. 1989. Pp. xx, 601. \$69.50.

There is probably no period in the history of Soviet industrialization more important than that of the first Five-Year Plan, which began in the late 1920s. The decade from 1928 to 1938, especially the first half, was the formative period of the Stalinist command system. The organizational arrangements and policy imperatives developed and implemented during these years are of interest to a wide range of scholars, not least historians and economists and, possibly most important, those attempting to understand the difficulties of change in the contemporary Gorbachev era.

In recent years, a number of scholars, among them R. W. Davies, have begun an in-depth reexamination of many facets of the early years of the Soviet economic experience. This effort, including the volume at hand, will doubtless improve our understanding of these early years and contribute to the continuing discussion of whether Stalin was "necessary," to paraphrase the language used over the years by Alec Nove and others.

The present volume is the third in the series by Davies, the first and second volumes dealing with collectivization and the collective farm respectively. In this third volume, Davies is concerned with a detailed examination of developments from mid-1929 through the end of 1930, the formative period for the first Five-Year Plan, and, most important, the arrangements and policies that would exist for years thereafter.

The primary objective of the author is to focus on and to analyze the alternatives available to Soviet leaders at the end of the 1920s and thus to enable a better assessment of the justifications for the path chosen by Stalin, in light of Soviet objectives and constraints at the time. A lesser theme, but one of substantial current relevance, is the extent to which better knowledge of the pressures of early Soviet industrialization and the responses to these pressures may facilitate our understanding of the Gorbachev era yet unfolding.

The discussion is prefaced with a summary view of the state of the economy at the end of the New Economic Policy (NEP) period and terminates with substantive conclusions about the nature and impact of the developments during this critical period.

This work is thoroughly researched, and the author does an excellent job of presenting much detail about almost all facets of the Soviet economy during this period. Unquestionably this detail will be fodder for those continuing to analyze these early years. At the same time, an effective summary at the end presents balance. The author points out, however, that it remains difficult to place weights on the various forces contributing to the Soviet reality of these years: the view that a rapid tempo of industrialization was essential, conditioned by ideology, foreign influences, and a strong tsarist legacy. Davies concludes that "no

one factor provides a sufficient explanation" (p. 464). Moreover, some will not agree with the author's view that "rapid industrialization was incompatible with the market economy of NEP" and that objectives of the leaders required "some kind of administrative planning system" (p. xviii).

The author notes, in the introduction, that an objective of this study is to better understand contemporary events through analysis of their origins. Although the reader (appropriately in this volume) is left to make the connection, it is persistent and striking, almost detracting from one's ability to sustain attention on the fundamental focus of the volume. Soviet leaders of the 1930s were bedeviled by a search for organizational arrangements with a magic solution to labor productivity problems to be found in part from exploiting "hidden reserves." The attempt to limit alcohol production and consumption was abandoned in the face of fiscal necessity. The list is long and, in places, one could imagine this discussion taking place not in 1929 but rather 1989. If the purpose of analyzing the past is to better understand the present, then this book will be important reading for all interested in the Soviet industrialization process and both its short-term and long-term outcomes.

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NEAR EAST

ROBERT OLSON. *The Emergence of Kurdish Nationalism and the Sheikh Said Rebellion, 1880-1925*. Foreword by WILLIAM F. TUCKER. Austin: University of Texas Press. 1989. Pp. xix, 229. \$35.00.

This study by Robert Olson is the first book in English to investigate a pivotal and little understood part of the transition to the modern Middle East. It is important to scholarship on the history of the Kurds, the Turkish republic, and British imperialism in Iraq.

Olson's initial chapters are a history of the interaction between the Kurds of Anatolia and northern Iraq and the Ottoman empire, the Turkish republic, and the British in Iraq. His section on the rebellion of Sheikh Said contains much material that has not been considered previously, particularly the interaction between the Azadi congresses of Kurdish nationalists and the more tribal and religious leadership of the nomadic Kurds. The place of the British in Kurdish affairs is also illuminated. For this information, the book is valuable to present understanding and future histories. The only caveat is Olson's sources, which are mainly British intelligence and Colonial Office reports. Given the present state of archival information, it is hard to see how Olson's sources could have been more complete, but one must be careful in taking British data at face value. Much other British intelligence from the period (such as reporting on the Turkish nationalists, 1919-22) has been deservedly called into question.

The book is valuable as descriptive history. The intent of the author goes beyond description to the issue stated in the title, but the major question of the book is unanswered. Was the rebellion of Sheikh Said a nationalist rebellion? To answer the question, Olson would have been forced to define nationalism, in the Middle Eastern context no mean feat. He does not attempt to do so. In Europe, nationalism was a combination of literary, philosophical, economic, and religious impulses that came together to form a national identity among a large group of people. The French were French, after all, and the Germans German not because kings, ministers, or intellectual revolutionaries said they were but because a large group of people demonstrably felt themselves to be French or German. For this reason, appeals to nationalistic zeal were successful calls to war.

It was not thus with the Kurds. Undoubtedly, some of the leaders of the rebellion were driven by nationalistic sentiments. As Olson has written, however, "While the Sheikh Said rebellion was a nationalist rebellion, its mobilization, propaganda, and symbols were those of a religious rebellion" (p. 153). In other words, those who followed their leaders into rebellion did so because of religious motivation. Many also followed tribal loyalties. The two loyalties, religious and tribal, were so intertwined as to be inseparable. Olson admits that banditry, tribal feuds, and personal vendettas were "prominent causal factors in the rebellion" and that the Kurds of the cities did not support the rebels. Kurdish nationalism was thus not a nationalism in the European sense, not a nationalism of the people.

Olson's ascription of the term "nationalist" to the rebellion rests on two points—the nationalistic character of the Azadi congresses and the desire of Sheikh Said to create an independent Kurdistan. That is as far as nationalism went. More convincing evidence would be needed before a basically European ideology such as nationalism could be attributed to the inhabitants of eastern Anatolia in the 1920s.

Olson's description of the important place of the rebellion in the development of the Turkish republic is much more firmly grounded. He accurately states that the religious character of the rebellion was of use to Kemal Atatürk in his drive to secularism. Whether or not the rebellion was a nationalist one, it is obvious that the Turkish government treated it as a religious rebellion that justified actions against the traditional place of Islam in government and society.

This book deserves reading, both for its history and as one element in the debate on Middle Eastern nationalism.

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GERSHON SHAFIR. *Land, Labor and the Origins of the Israeli-Palestinian Conflict, 1882–1914*. (Cambridge

Middle East Library, number 20.) New York: Cambridge University Press. 1989. Pp. xvi, 288. \$44.50.

Gershon Shafir begins his study by noting that it "was engendered by the dislocating experience of growing into maturity as part of the Israeli generation of 1967" (p. xi). This work contributes to an emerging literature that revises earlier accounts of Israeli development in light of recent history as well as access to new archival materials and comparative theoretical frameworks. It is based on the author's belief that "historical sociology should be viewed as an integrated enterprise of historically grounded theory formation, i.e. the generation of theoretical propositions and concepts by means of the analytic ordering of the past in its relation to the structures of present-day society" (p. xiii). Accordingly, Shafir engages in a dialogue primarily with other Israeli sociologists and historians about the process of state and nation formation in modern Israel.

Basic to this study is the argument that modern Jewish settlement in Palestine must be understood in its own terms rather than through the prism of Zionist ideology. Shafir argues that, from the beginning, conditions in Palestine itself determined choices regarding land, labor, and patterns of settlement. These in turn helped create the institutions and organizations central to the modern state of Israel. Thus, Shafir seeks to convince the reader that "what is unique about Israeli society emerged precisely in response to the conflict between the Jewish immigrant-settlers and the Palestinian inhabitants of the lands" (p. 6). It is this perspective that guides the author's work and gives his argument coherence. Although others may disagree with his choice and use of the evidence, this book is a very cogently and intelligently argued presentation. Shafir succeeds both in conceptualizing the discussion of Israeli history in direct relationship with that of the Palestinian Arab community and in showing that our understanding of the Jewish community of Palestine is singularly distorted by studies that ignore the impact of immediate conditions on its evolution.

The author chose to limit his study to the last period of Ottoman government in Palestine. In his survey of this period, Shafir seeks to place Zionism in the context of outside intervention by a variety of forces in the Ottoman empire and the patterns of economic dependence that developed. This perspective allows Shafir to focus on his primary concerns of land acquisition and labor employment as providing the foundation for continued Jewish settlement in Palestine. Moreover, Shafir's analysis permits him to point out and discuss alternative positions taken within the Jewish community itself with regard to policies of labor employment and agricultural cultivation. His chapter on the role of "natural workers" from Yemen between 1909 and 1914 is particularly suggestive in arguing his central thesis that "the status and class position of Jewish groups in early Israeli

society was bound up, and maybe for some groups and strata still is, with the broader national conflict between Jews and Arabs" (p. 120). Far from being independent of that conflict or in a position to ignore the Arab population, Shafir argues that awareness of competition with Arab labor and vulnerability to economic forces shaped the choices made by earlier settlers and helped determine which groups would become dominant in the emerging nation and eventually in the state.

In the last several chapters of the book, Shafir extends his analysis of land and labor conditions to show that the emergence of trade unions, political parties, cooperative settlements, and military forces must also be understood in the context of the specific conditions with which their membership was dealing in Palestine and within the Jewish community itself. Although this discussion is necessarily limited, it is sharply focused again on the author's central argument. In the concluding chapter, Shafir attempts to link his historical presentation to the evolution of the Israeli-Palestinian conflict and specifically the role of the labor movement in that conflict. His position is clearly stated and reminds the reader that this is a study undertaken with a view to understanding the past in its relation to current Israeli realities. Shafir's work makes a notable contribution to the literature on Israeli history and development by broadening the framework of discussion.

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AFRICA

DAVID PROCHASKA. *Making Algeria French: Colonialism in Bône, 1870–1920*. New York: Cambridge University Press. 1990. Pp. xix, 328.

This title can properly be taken as ironic, and the author concurs (p. xviii). David Prochaska's book is almost wholly about the settlers rather than the French colonial authorities or the Algerians. It is they who he claims were "making Algeria French." Prochaska attributes the irony to his belief that the settlers achieved much of this aim in the eighty-five years or so (1870–1954) when their wishes were paramount—only to have the Algerian revolution put a relatively early end to their creation by 1958. He calls this "a case of the failure of success" (p. xviii). This reviewer maintains that the settlers never succeeded in any important way. Save in regard to appearances, such as official architecture and public ceremonies, they failed. In respect to a French principle as fundamental as the rights of man, the settlers never came close to a *conquête des coeurs* based on the consent or at least the tolerance of the governed.

In point of fact, Prochaska demonstrates all of this. He does not gild over the selfish and cynical behavior of the settlers operating through economic exploita-

tion, urban segregation, and electoral manipulation abetted by political alliances in the French parliament. He writes, "the settlers blocked social evolution, attempted to contain history, and precluded thereby any genuine rapprochement with the Algerians in the twentieth century" (p. 26). But he adds that "the settlers formed a colonial society and in the process made Algeria French, at least before the rise of Algerian nationalism" (p. 27). Beginning his work with a section on precolonial Annaba (the Arabic name for Bône), the author continues with a chapter on the transition years after the French conquest in 1830 and follows with other chapters on the full-fledged settler colonial city (1870–1920), "when the Algerians tend to drop out of the picture" (p. 231), and on the revolt and collapse at the end (1954–58).

This is a substantial contribution to a growing field of research: the evolution of colonial urban centers in various imperial settings. The author draws on earlier works and proposes conceptual refinements. Why was Bône chosen? It was partly because of the anomalies the city presents. The author summarizes it best: "Bône is a settler colonial society in which the European colonizers outnumber the Algerian colonized two to one. A French colony in which the Italians, Spanish, and Maltese are as numerous as the French. . . . A French colonial society created largely by naturalizing Jews and Europeans" (pp. 177–78).

In addition, Bône afforded a fine research tool: "a virtually continuous run of censuses from 1848 to 1960" (p. 178). The resulting computerized study is one of the largest data bases drawn from Arab-Berber North Africa and "perhaps the only census sample based on sources for the colonial period" (pp. 178–79). Bône was also the capital of an area rich in natural resources: cork, iron ore of the Ouenza, and phosphates of Tebessa. These were rapidly exploited by the new arrivals or their principals. Smaller businesses also came increasingly into non-Muslim hands between 1885 and 1915 (p. 126).

VINCENT CONFER,
EMERITUS
Syracuse University

LASZLO J. NAGY. *La Naissance et le développement du mouvement de libération nationale en Algérie (1919–1947)*. (Studia Historica: Academiae Scientiarum Hungaricae, number 190.) Budapest: Akadémiai Kiadó. 1989. Pp. 171. \$18.00.

The subject of this study would be more clearly stated if the words "with special attention to the role of the Communists" had been added to the title. This focus is both the strength and the weakness of the book. In Algeria (and for that matter Morocco and Tunisia, too) many French and other European settlers brought their proletarian ideologies and organizations with them and found recruits among the ranks of the colonized. That factor produced a colonial

environment different from what prevailed in areas where few Europeans settled. It is useful to have a detailed account of the emergence of Algerian nationalism as seen from this perspective.

Moreover, the author has consulted the standard works on Algeria but has also pushed on to use the memoirs, party journals, and newspapers as well as other materials written by members of the Algerian Communist party or of the party in France. This is a strength of the book. Another strength is a coherent linking of French Algeria's economic situation to a Marxist class analysis.

The basic format of the book is chronological with a solid introductory chapter followed by chapters treating, in order, the 1920s, the 1930s, the Vichy period, and the final era of an emerging post-Vichy Algeria, 1943–47.

Most studies trace Algerian nationalism in terms of three different streams associated with the “proletarian” radical nationalism of Messali Hadj (with ties to Algerian workers in Europe), the bourgeois “moderates” represented by Ferhat Abbas, and the reformist Algerian Association of Ulama organized by Bin Badis. The Communists, whether French or Algerian, are given a lesser role.

Laszlo J. Nagy disputes this. In his preface he takes issue with scholars (naming Charles Robert Ageron and Emmanuel Sivan) whose studies view the Communist record in Algeria as a series of setbacks. The author, to his credit, does not attempt to justify all Communist party activities. He highlights, for example, the party's tardy espousal of Algerian independence. I, however, am left with the feeling of having learned much about Communist party policy in Algeria from World War I to 1947 without being persuaded that the more standard interpretations of Algerian nationalism need to be modified.

L. CARL BROWN
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ROBERT H. BATES. *Beyond the Miracle of the Market: The Political Economy of Agrarian Development in Kenya*. (Political Economy of Institutions and Decisions.) New York: Cambridge University Press. 1989. Pp. xv, 203. \$37.50.

At a time when leftist scholars tend to label themselves “post-Marxist,” “poststructuralist,” or “post-modernist,” Robert H. Bates—a distinguished mainstream political scientist and a critic of African governments' interference with the market—has written a book whose perspective might be labeled “post-neoclassical.” Like his colleagues to his left, Bates claims to have “been there,” to have worked within and now beyond a body of theory. He seeks to convince the faithful among market-oriented economists that their focus is too narrow and that they need to analyze not just the calculus of market relations but that of political relations as well. Institutions—from

the formal apparatus of political parties and administrations to the more subtle workings of lineage politics—need careful examination if we are to understand how markets operate in any given situation. Institutional structures both reflect and shape economic processes.

The historian whose professional bias is toward the study of institutions or the Marxist or dependency theorist might find this post-neoclassical approach a welcome sign that someone such as Bates is coming around. Bates's “political economy” is not all that dissimilar from the other brand. At times, his enterprise seems like one of translation; the substantial literature on Kenya by anthropologists, historians, and critical political scientists is put into a language that economists might understand. But smugness is not in order. Bates's theoretical reading has been wide and his observations on different points of contention are insightful, interesting, and original. He is specifically critical of some of the interpretations of Kenyan history put forward by radical political economists. Yet his approach shares the problems of a very non-post-Marxist. It is resolutely and explicitly materialist. Just as Marxists often fail to analyze the connection between the class character of production and the class character of politics—because they assume it—Bates's rational calculus of political economic decision making does not consider how political actors interpret their universe or how political culture affects and is affected by ongoing conflicts. He is good at explaining the logic of individual decision makers but vague about collectivities. He is as willing as an old-style Marxist to say Kenya was ruled by a property-owning class, but he does not tell us how the classness of that class emerged, how its boundaries were delineated, and how it defined its collective identity and political coherence in the face of cross-cutting ties and identities. Collectivities emerge as actors without full explanation of why they are collectives or how they act.

Such missing links are found in the most historical section of the book, his interpretation of Mau Mau. Bates's material is not new, but he argues in a clear and rigorous manner. The significance of Mau Mau, for him, lies in the process of its defeat. The “aggressive elites of the Kikuyu reserves,” who had been accumulating land and shedding traditional ties and obligations, helped the colonial state defeat the rebels and obtained more access to its coercive apparatus, positioning themselves to triumph in the succession. Jomo Kenyatta came to power allied with the “incipient gentry” of central Kenya (p. 89). This becomes the key argument of the book: Kenya's government, unlike those of most African states, is not tied to an urban bourgeoisie but to a rural, farming elite. But a class is labeled “gentry” with little more said about it than that it is based in the country. How such an entity came to act collectively remains a mystery. We also do not understand its boundaries: Bates's argument would explain why both Mau Mau and its

enemies would tend to be Kikuyu, not why both were constituted in specifically Kikuyu social and cultural terms.

With his gentry in power, Bates moves to a series of studies of how political institutions are formed and how their nature as institutions affects the ways in which markets operate. Although not a fully integrated interpretation—and one with major lacunae, including the entire international dimension of political economy—the book illustrates effectively the ways in which institutions created to improve capital markets or protect against famines serve also to enhance the power of particular elites and redistribute income in their favor. Bates denies (albeit in a note on page 169) that the regulatory interventions of the Kenyan state can be understood as part of a “dirigiste dogma” but argues instead that they follow a complex political-economic logic. Interestingly, Bates started out trying to show, through his analysis of the defeat of Mau Mau, why the Kenyan state was different; he ends up suggesting that since the death of Kenyatta, it is becoming more like the majority of African states.

If Bates can explain the political-economic rationality of Kenya's decision makers, he does not explain what produces such a general and powerful convergence. At the same time, his extension of the mode of thought of economics to the arena of politics provides telling insights but leaves numerous puzzles. As is the case with post-Marxists, it is more apparent where this post-neoclassical interpretation is coming from than where it is going to.

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JOYCE AVRECH BERKMAN. *The Healing Imagination of Olive Schreiner: Beyond South African Colonialism*. Amherst: University of Massachusetts Press. 1989. Pp. xi, 317. \$30.00.

The last decade has witnessed a resurgence in scholarly interest in Olive Schreiner, a South African writer of fiction and nonfiction who lived from 1855 to 1920. She was a complex person, with complicated political views often ahead of their time. Her *Women and Labour* (1911) appeared in bibliographies circulating in the early 1970s as another generation of feminists looked for female authors and theorists to articulate the nature of and solutions to women's problems. Joyce Avrech Berkman's study of Schreiner's writing and thought makes clear the relevance of her work for present-day feminists, as well as those of two decades ago and Schreiner's contemporaries.

Schreiner's ideas developed in the context of personal struggles, political conflicts, and intellectual currents. She hoped to become a doctor, but her ambitions were thwarted by debilitating bouts of asthma. Raised in a missionary family, she rejected the guilt and damnation approach of Christianity,

while retaining a profound spiritual orientation. Affirming female sexual passion, she sought an egalitarian and fulfilling marriage as well as nonsexual heterosexual relationships with such leading sexologists as Havelock Ellis and Edward Carpenter. Writing from an outpost of empire and opposing the British in the Anglo-Boer War, she experienced the British sense of superiority. She articulated anti-imperialist politics and a remarkably egalitarian vision of nonhierarchical society, linking in her critique domination based on race, sex, class, and nation. In her overall assessment of Schreiner's thinking, Berkman notes that what sustained Schreiner in her intellectual, political, and personal endeavors (and, indeed, she struggled on all fronts) was optimism—an optimism that blinded her to the intractable realities of the problems she addressed and the possible need for violent response anathema to Schreiner as a pacifist.

While affirming her respect for Schreiner, Berkman does not avoid criticizing her inconsistencies and weaknesses. Schreiner's analysis of sexism, for example, was more penetrating than her analysis of racism. And, despite her critique of Social Darwinism's racist depiction of evolution that resulted in European society at the apex, she slipped into facile ethnic characterization.

Berkman does an excellent job of situating Schreiner's ideas vis-à-vis those of her contemporaries. Unlike many feminists, she came to reject the notion that women were, by nature or by virtue of socialization, more inclined to pacifism than men. Moreover, she argued that men and women were substantially alike in terms of “erotic appetites and parental impulses” (p. 92). Unlike key theorists such as Friedrich Engels, she envisioned early human society as patriarchal, drawing on her knowledge of male dominance in South African ethnic groups. Although accepting an evolutionary framework, she viewed all races as evidencing a mixture of positive and negative traits.

Committed to careful examination of Schreiner's ideas, Berkman does not focus on chronological narration and reconstruction of the biographical details of Schreiner's life. Other biographies are available for that purpose, and Berkman outlines where she disagrees with their interpretations and with those of literary critics.

MARGARET STROBEL
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ALAN GREGOR COBLEY. *Class and Consciousness: The Black Petty Bourgeoisie in South Africa, 1924–1950*. (Contributions in Afro-American and African Studies, number 127.) New York: Greenwood. 1990. Pp. xiv, 258. \$45.00.

This solid and rich monograph argues against the common leftist dismissal of petty bourgeois false consciousness and compromising failure. Alan Gre-

gor Copley describes his subjects as "a vibrant and resilient group characterized by dedication, ingenuity and achievement in the face of staggering odds" (p. xi). Copley's survey covers economic conditions and business ventures of the predecessors of the African National Congress (ANC), their land purchases and credit schemes, their cooperative movements and programs for "racial uplift" in conjunction with white liberal patronage. Almost as a rare by-product, the study contains a balanced analysis of the myriad missionary activities in South Africa as well as the African-American influence, particularly through Marcus Garvey. Populist Garveyism resembled the black consciousness movement of the 1970s. The belief in "race solidarity," in a heroic African tradition, expressed in a "flamboyant race pride" always competed with the nonracial emphasis, inspired by socialist internationalism, liberal individualism, and universalism. The nonracial tradition has won the drawn-out battle for the moment, but its victory remains fragile and dependent on its ability to deliver.

Copley is at his best when dealing with the consciousness and identity of the petty bourgeoisie. He relates fascinating accounts of the early ideological disputes and personality clashes. For example, in 1930 the Communist party—currently the staunchest ally and mainstay of the ANC—concluded: "The African National Congress is now openly a servant of the imperialist bourgeoisie and uses its endeavors to damp down the revolutionary activities of the masses" (p. 197). The present emerging alliance between the ruling National party and the ANC tempts the question whether a potential break-away Communist party and union movement in opposition to an ANC-led government could repeat that judgment sixty years later. Copley conveys well the past fractious struggles within the resistance. The Stalinist past of the South African Communist party (the support for a native republic, conceived in Moscow; the initial opposition to the Allied war effort; and the subsequent reversal when Hitler attacked the Soviet Union in 1941) and the ambiguity about a racist white working class still haunt political activists today.

Copley is less convincing when trying to locate and define the petty bourgeoisie conceptually. He presents scant summaries of the vast literature (Leo Kuper, Thomas E. Nyquist, E. A. Brett, Phil Bonner, Helen Bradford, Lynette Dreyer) more as contrasts to carve out a theoretical niche for himself rather than to exploit the insights of predecessors. Copley's own empirical material contradicts his emphasis on "the extraordinary degree of social cohesion" (p. 91) and common identity of a vastly heterogeneous group. In trying to define the petty bourgeoisie "in a context of class struggle"—an intermediate class, aspiring, structurally ambiguous, fractured, and susceptible to proletarianization as well as eager to "identify upwards"—the author falls back into Marxist jargon that he usually avoids. He comes close to crude reduction-

ism when he concludes that members of the petty bourgeoisie, "because their position is dependent on the dominant classes in struggle . . . are forced to express their consciousness as a class in terms borrowed from those classes" (p. 3).

Nonetheless, what emerges unequivocally is that the nonracial, moderate, cooperative liberalism of the black leaders in the 1920s and 1930s did not impede black radical opposition. "The suggestion that they were 'duped' into supporting a strategy of white domination and black subordination seems as much an insult to their intelligence as it is at variance with the facts" (p. 91).

HERIBERT ADAM

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JAMES BARBER and JOHN BARRATT. *South Africa's Foreign Policy: The Search for Status and Security, 1945–1988*. (Cambridge Studies in International Relations, number 11.) New York: Cambridge University Press, in association with South African Institute of International Affairs, Witwatersrand, South Africa. 1990. Pp. ix, 398. Cloth \$54.50, paper \$16.95.

In 1960, following the international condemnation of South Africa for the shooting of unarmed protesters at Sharpeville, the Afrikaner newspaper *Die Burger* lamented that "it is a bitter thing to be forced into the role of the skunk of the world." Since its adoption of apartheid in 1948, South Africa has been a "global skunk" and has faced the problem of dealing with an increasingly hostile world. This comprehensive and insightful book examines Pretoria's relations with an international community gradually unified in its opposition to South Africa's institutionalized racism. The depth and analysis of this study make it the standard work on the topic, superseding previous studies such as Amry Vandembosch's *South Africa and the World* (1970), and Deon Geldenhuys's *Diplomacy of Isolation* (1984).

James Barber and John Barratt make it clear that the dominant goal of South Africa's foreign policy for the past four decades has been "the preservation of a white controlled state" (p. 1). The authors demonstrate, however, that Pretoria has used a variety of strategies to try to gain this objective. Barber and Barratt contend that there were alternating periods of confidence and assertiveness followed by eras of doubt and insecurity in South Africa's foreign relations. The major cause of these vacillations has been the level of domestic protest and the resulting foreign pressures on the nation. In times of relative stability at home, South Africa was quite successful in gaining foreign investment and international acceptance and in blunting criticism of the nation. When internal opposition to apartheid was visible and strong, its diplomacy was generally ineffective.

The book has numerous strengths: it is chronologically and geographically comprehensive. The au-

thors examine South Africa's relations with Europe, the United States, the United Nations, and, most welcomed, with other African nations over a span of four decades. The book is also clearly written. It effectively leads the reader through the machinations of both white and black politics in South Africa and the changes in policy with each new government. Finally, Barber and Barratt show the complexity of Pretoria's diplomacy in pursuing economic growth, military power, and international legitimacy while attempting to control an ever larger and more militant black population.

The book is marred by some annoying typesetting, proofreading, and spelling errors (Nikita Khrushchev as Kruschev; Arthur Goldberg as Golberg; G. Mennen Williams as Menon Williams; Zbigniew Brzezinski as Brezezinski; Avery Brundage as Bundage; Kwame Nkrumah as Nkruma), but these are minor distractions in a quality work. Impressively researched, tightly organized, and highly readable, this book is an excellent study of a complex and timely topic.

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ASIA

THOMAS J. BARFIELD. *The Perilous Frontier: Nomadic Empires and China*. (Studies in Social Discontinuity.) Cambridge, Mass.: Basil Blackwell. 1989. Pp. xiii, 325. \$38.95.

This volume is a welcome addition to the literature on the relations between Central Asian empires and China in premodern times. Tracing the history of the nomadic empires and their relations with China, Thomas J. Barfield provides us with stimulating interpretations. His thesis is that the nomadic empires, maintained by resources supplied by the agrarian empires such as China, developed an "outer frontier strategy" of violently raiding for loot to terrify the Chinese court, alternating peace and war to increase subsidies or trade, and refusing to occupy Chinese land (p. 88). With the exception of the Mongols, the nomads generally avoided conquering Chinese territory because they needed a stable China to exploit. The Mongols did not initially want to conquer China but were drawn into China by the Jurchen Chin, a foreign dynasty that refused to accommodate them. Moreover, the policy of the nomads toward China, best exemplified by that of the Uighurs toward the T'ang, even provided support for weak Chinese governments. The author argues that the nomadic empires were so dependent on Chinese resources that unifications of the nomads and of the Chinese at the same time were not coincidental; when the nomads were forced to depend on their own resources, their political system collapsed (pp. 90-91). It seems that the best foreign policy for China was one of appeasement under the cloak of the tributary system because

paying off the nomads was a cheaper and better strategy than constantly fighting them. With these themes in mind, the author also describes the rise and fall of the nomadic as well as the "Manchurian" empires with interesting analyses of their political systems and succession patterns.

Although critical of Owen Lattimore's thesis that the nomads often invaded China when China was weak, Barfield's point on the successful Manchurian empires of the northeast does remind us of Lattimore's thesis that Manchuria was a reservoir of potential rulers of China, such as the Jurchen and the Manchus, who used a dual political system by employing Chinese officials and who understood both the strengths and the weaknesses of the nomadic and agrarian societies so that they were able to encroach on and even conquer China. Barfield elaborates that the Hsien-pei, the Khitans, the Jurchen, and the Manchus from the Manchuria region were successful in the complex three-way interaction between the Chinese, the Manchurians, and the nomads. The Manchus, for example, kept the nomads of Mongolia in a state of anarchy and manipulated the tribes, preventing the rise of steppe empires.

The author uses secondary works in Western languages and translations from Chinese materials and does not consult much modern Chinese and Japanese scholarship on Chinese foreign relations. Naturally he is not aware of works with similar arguments. For example, Sechin Jagchid already contended some time ago that traditional China's best policy toward the nomads was appeasement together with trade that supplied the nomads with the necessary resources (*Peace, War, and Trade between North Asian Nomads and the Agrarian Chinese* [1973], in Chinese). It is rather curious, however, that Barfield never mentions René Grousset's *The Empires of the Steppes: A History of Central Asia* (1970). Barfield would definitely disagree with Grousset's statement that "the paramount fact in human history is the pressure exerted by these nomads on the civilized empires of the south, a pressure constantly repeated until conquest was achieved" (p. xxv). But how similar is Grousset's observation that the nomad "roved on horseback along the fringes of ancient empires, exacting regular tribute from those who complied with a relatively good grace, or, when the victim was ill-advised enough to refuse payment, plundering open cities in sudden raids" (p. xxvii)? Also, the term "Tungus" never appears in Barfield's book, and no distinction is made between the proto-Mongol Hsien-pei and Khitans and the Tungusic Jurchen and Manchus. All of them are loosely considered as "Manchurians." The Khitans, however, seem to have employed the "outer frontier strategy" to cope with China throughout the Liao period.

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MIN TU-KI. *National Polity and Local Power: The Transformation of Late Imperial China*. Edited by PHILIP A. KUHN and TIMOTHY BROOK. (Harvard-Yenching Institute Monograph Series, number 27.) Cambridge: Council on East Asian Studies, Harvard University, and the Harvard-Yenching Institute; distributed by Harvard University Press. 1989. Pp. 309. \$26.00.

In their preface, the editors remark that Min Tu-ki is like a fellow traveler who "has certain special insights and talents which have enabled him to see things [we] have not seen, and to understand more deeply those things [we] have both seen" (p. v). Thus, although five essays in this volume of translations first appeared in Korean before 1970 and another essay appeared in 1978, they have not been superseded by subsequent scholarship and remain instructive, stimulating reading.

Min's first essay on Pak Chi-won's sharply observed "Jehol Diary" provides new material as well as new insights. Traveling through north China in 1780, Pak described a paradoxical situation of weakened public order in the midst of increasingly severe imperial regulations that produced a fear so chilling that friends burned records of their conversations. Yet ethnic awareness and anti-Manchu feeling (neither typically associated with this period) still existed. Intriguing as Pak's account is, Min, other than suggesting that we can take one gentry source as representative of the gentry class, is properly reluctant to generalize on the basis of it.

Turning to the recurring question of the *sheng-yuan* and *chien-sheng*, Min reaches a conclusion similar to Chang Chung-li's but for different reasons. Min argues that the *sheng-chien*'s status as "schoolmen" not yet part of the examination system, their participation in tax engrossment and proxy litigation, their susceptibility to beatings by local officials, and their relative social immobility created a sense of alienation from those above and below as well as a common identity that marked them as a "middle stratum." Min's evidence for self-conscious collective action is thin, and his analysis excludes post-Taiping changes that made office holding and upward mobility more likely for these men. Nevertheless, his essentially cogent analysis is more dynamic and less normative than either Chang's or Ho Ping-ti's.

Min's writing lacks an explicit ideological bent. If there is a unifying theme to his work, it is the "danger of overestimating outside forces. . . . Change in China becomes an actuality only when it is rooted in Chinese history" (p. 177). The rigorous reexamination in chapter 3 of the "Chinese 'principle'/Western 'utility' (*t'i-yung*)" antinomy bridges the internal perspective of the first two essays and the interactive concerns of the last three. Arguing that everyone from Wei Yuan to Liang Ch'i-ch'ao took principle to be principal and utility to be both subordinate and Western regardless of whether it was technology or political institutions, Min sees no value in using ad-

herence to the *t'i-yung* construct as a yardstick for judging degree of progressiveness. Moreover, given the late Tokugawa's fascination with similar ideas and Japan's "success," he concludes that the concept (which he regards as likely to appear whenever "an alien culture is being forced on an indigenous one" [p. 86]) cannot be blamed for China's relative failure to implement institutional change.

The transformative tension between national polity and local power of the book title is the subject of the final three chapters that revolve around the evolution of the ideas of "public" and "private" as well as their expression in political thought and practice. Min's thorough review of Qing theories of political feudalism should become a standard. Although Min does not fully demonstrate intellectual lineages, he carefully tracks the idea throughout the dynasty rather than leaping (like most extant treatments) from Ku Yen-wu to Feng Kuei-fen. He reminds us, moreover, that these thinkers' motivations were not uniform: they comprised moral reasons as well as concern for institutional reform.

In the final two chapters, Min analyzes how the emphasis of political feudalism on the local and self-rule manifested itself in the provincial assemblies and in the Soochow-Hangchow-Ningpo railway dispute and how it clashed with the centralizing tendencies of the metropolitan government. Like Ernest Young and Stephen MacKinnon, Min takes particular note of Yuan Shikai's influence in the years before 1908. Indeed, compared to Mary Rankin's province-centered approach, Min pays relatively greater attention to events at the center during the railway dispute. Yet, whereas Rankin stresses continuity between metropolitan elite oppositional politics of the late nineteenth century and the railway movement, Min focuses on new social types. One cannot pursue these points here, but distinguishing Rankin and Min would be an excellent historiographical exercise for advanced undergraduates.

Since few American scholars of China speak Korean, I hope that more translations such as this one will be undertaken in the future. We owe a debt of thanks to Min, the editors, the translators, and the Harvard Council on East Asian Studies for an excellent start.

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BENJAMIN YANG. *From Revolution to Politics: Chinese Communists on the Long March*. (Westview Special Studies on China.) Boulder, Colo.: Westview. 1990. Pp. xiv, 338. \$33.95.

The decade after 1927 was an extraordinary time in China. While Chiang Kai-shek strove to consolidate his regime, the Communists grew from ragtag desperados to a confident army and party, poised for explosive expansion.

Pivotal events occurred between 1934 and 1936. In October 1934 the Communists' principal "soviet" was under deadly siege. By October 1935, the Long March had delivered them to a new base area in Shaanxi province. Other Communist forces were brought into an increasingly indomitable organization in October 1936. Mao Zedong began the Long March in political eclipse and emerged as principal leader and strategist. Benjamin Yang's fine book brings out the complexities of this story in the light of new documents. He illuminates important issues and offers a fresh narrative.

Several points stand out. Yang revisits the Li Lisan story and reevaluates the positive accomplishments of this maligned Moscow-trained leader. He analyzes Chiang's five annihilation campaigns and the Communists' crisis under the fifth campaign. He discusses Mao's eclipse in 1933 and provides a more modulated picture of Mao's position at the nadir.

Yang reviews the career of Zhang Guotao, Mao's major rival. He reassesses the famous Zunyi conference, arguing that it was not such a singular turning point as is commonly believed. He highlights the irresolution of the Long March (or Marches) and shows that the final destination was settled very late. Yang discusses the different marches of Zhang Guotao and others and argues that only in Shaanxi, under Mao's leadership, did the Communists mature.

One can compare this study with Harrison Salisbury's *The Long March: The Untold Story* (1985). Salisbury's more dramatic journalism makes the story vivid to a broad audience, whereas Yang, despite his sometimes awkward English and hopeless maps, provides the more sober, less reverent approach expected by specialists. The two books together provide a sound general understanding.

In his conclusion, Yang becomes more theoretical but less persuasive. His interesting argument that this period witnessed the transfer of the Communist movement from south to north China is pushed further than the definitional and empirical basis for it allows. What constitutes "north" and "south"? What about the exceptions? By Yang's own early analysis, Communist success, north or south, depended mainly on whether canny leaders were left relatively unchallenged in a given location.

According to Yang, after 1949 Mao pushed international revolution, even at the expense of China's interests and, in the Cultural Revolution, wanted to "shed his power" (p. 262). Neither assertion is persuasive. Yang predicts that in the near future political rather than economic construction will be primary and that China will experience political shrinkage. Again, none of this is well demonstrated.

The book's biggest unresolved issue is reconciling the pre- with the post-Liberation Mao Zedong. Yang says that the early Mao imposed "politics" (pragmatic policies) on a movement self-destructively inclined toward social "revolution." But after 1949, Mao himself reverted to "revolution." Yang offers little to

explain the transformation. Despite this weak chapter, however, this book is an important contribution to the field and belongs in every specialist's library.

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JEFFREY P. MASS. *Lordship and Inheritance in Early Medieval Japan: A Study of the Kamakura Sōryō System*. Stanford: Stanford University Press. 1989. Pp. viii, 332. \$38.50.

The first intensive Western-language study of family authority in thirteenth-century Japanese warrior organization, this book focuses on the so-called *sōryō* system, in which a senior member of a warrior family was authorized to represent the whole when the Kamakura government levied service taxes on its lands. Within the group, each member had his (or her) own holdings, and inheritance was partible, but for certain purposes respecting outsiders, one person, the *sōryō*, was regarded as "holding all" (the original meaning of the Japanese term). About fifteen years ago, the significance of this institution was vigorously debated among Japanese scholars, but few definite conclusions were reached. Jeffrey P. Mass reviews the major arguments and presents a synthesis of his own, buttressed with citations from hundreds of testamentary documents and judicial decrees.

Mass brings to his documents a framework of assumptions about "Japanese feudalism," largely developed by J. W. Hall, stressing vassalage as a defining characteristic. Because dominion over land and obligations of military service were not, during the Kamakura period, anywhere nearly congruent, the warrior regime at Kamakura was not, in this view, truly feudal. Within the vassal family, senior members attempted to reduce their lesser kin to "feudal" subjection by preempting control over their inheritances. The Kamakura *Bakufu*, on the other hand, to protect its hegemony, inhibited the consolidation of large corporate warrior households mainly by providing a special judicial system that facilitated litigation among kin. Meanwhile, linkages based on kinship weakened, partly because of the dispersal and resettlement of warriors occasioned by the many wars, and landed property became the chief basis of power in the warrior band. Just as in Western Europe, progress toward "feudalism," despite *Bakufu* efforts to arrest it, was inexorable. The case for "Japanese feudalism" is further supported by the strong suggestion in chapter 5 that, without the influence of the church in medieval Europe, inheritance practices there would have been like those of Japan. These broad generalizations raise a number of critical issues and should arouse greater interest in this field among institutional historians generally.

The study devotes considerable attention to the power of women to hold land titles and transmit property independently of husbands or male collat-

erals. It explains the circumstances prompting fathers of the late thirteenth century to restrict bequests to daughters to life estates with remainders retained in the male line. Although the argument that life estates for women were the pre-Kamakura norm is weak (p. 19), the survey of Kamakura adjudications on women's estates is particularly well done and is more than enough to establish this book as a landmark work. As in Europe, Mass points out, women could meet military obligations through substitutes, but they were also free to adopt successors of their own. This book provides ample proof that, during the thirteenth century, inheritance of property was generally bilateral and that a married woman was not legally absorbed into her husband's lineage.

A study of this scope and depth of detail must necessarily pose more problems than it can solve. The documents translated are often quite difficult, and significant slips occur. There is, for example, confusion over the meaning of the term *sōryō* in documents 11 and 27 (pp. 63 and 64). As Mass himself points out, this word was routinely used before Kamakura times to mean either "the entire holding" or "to take all" or "to hold all" and only later acquired the meaning "house chieftain." In attempting to find the earliest unambiguous usage of the word in this derived meaning, one should eliminate all cases where context permits other interpretations. The phrase in document number 11 translated as "Motoshige was *sōryō*" probably means "the entire holding of Motoshige," "Motoshige" being the name of a landholding, not a person. In number 27, *sōryō* seems to mean "to take all" rather than "to be house chieftain," at least to me. Examination of the sources cited by Mass will, I think, confirm the view generally accepted among Japanese scholars that, starting in the 1220s, *Bakufu* authorities and testators of landholdings devised a legal fiction whereby the division of an inheritance among children would be qualified by the designation of some son as representing the corporate group as if he were "holding all." Similar difficulties appear in number 75, cited on page 73 to show how the *Bakufu* might have been willing to waive its own statute of limitations to uphold the testament of a long dead father. This text is not, however, a legal decision at all. It is merely an instruction by Kamakura to a lower court to investigate further a defendant's claims that the testament asserted against him was not genuine and that he had already been in possession for the prescriptive period of twenty years.

In connection with this misreading, one can almost detect a prejudice against statutory law. Mass barely mentions the voluminous Kamakura legislation on family, property, and inheritance. Why omit the 1295 ruling that confiscation of property for the criminal acts of a *sōryō* would not extend to his kin or the earlier statutes limiting the alienability of vassal holdings? Given Mass's emphasis on political strategy as a driving force of history, legislative policies should have gotten more extensive treatment.

The formally institutionalized *sōryō* was clearly the legal artifact of a bureaucratic administration, but the question remains whether, or to what extent, the institution reflected an underlying mode of corporate proprietorship already present among local elites before the establishment of the *Bakufu*. An examination of the connections between the warriors and the proprietary cultivators just beneath them in the regional hierarchy, already examined by Japanese scholars, would be a welcome sequel to this excellent study. Despite occasional inaccuracies, its analysis of succession and property among Kamakura vassals is original and provocative, and its elucidation of the ambiguities of the *sōryō* system throws new light on the relation between state and property in a feudalistic society.

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MIKISO HANE, editor. *Reflections on the Way to the Gallows: Rebel Women in Prewar Japan*. Berkeley and Los Angeles: University of California Press. 1988. Pp. viii, 275. \$22.50.

One might say that the first serious approach by Japanese scholars to the study of women was at the beginning of the modern age, that is, in the early Meiji period. Fukuzawa Yukichi was one of the first to address this problem, and, indeed, in 1988, the University of Tokyo Press made an important contribution to the field of Japanese studies by publishing *Fukuzawa Yukichi on Japanese Women: Selected Works*. But Fukuzawa's writings addressed themselves mainly to middle- and upper-class Japanese. Mikiso Hane's book on Japanese women is also important. Hane's subjects came from very diverse backgrounds, but they shared one thing. They were modern Japanese women who resisted gender- and class-based oppression and who were subsequently repressed, imprisoned, and even executed for their political beliefs. Hane's goal is for us to hear these previously unheard voices of early modern Japan.

The author chose his subjects not only for the role they played in reform movements before World War II but also because they were so candid in revealing their thoughts and in accounting for their actions. In Hane's view, they "played roles as significant as those of the male activists in laying the groundwork for the continuing struggle to extend human rights and ensure social justice for all members of the society" (pp. 3-4). Ironically, a leitmotif in many of the accounts is that these women often found themselves betrayed by their men, some of whom saw no contradiction in their professed humanitarian ideals and their exploitation of women.

Hane begins his work with a historical survey of Japanese women, showing how along the way they became subjected to the manipulation of what psychologists today call "learned helplessness" and

"learned dependence." He then provides six portraits of rebel women, including individuals and groups. For me, the most fascinating accounts were those of Fukuda Hideko (1865–1927), a product of the "popular rights and freedom" movement of the late nineteenth century; Kanno Sugako (1881–1911), hanged because of her involvement in a plot to assassinate the Meiji emperor; and Kaneko Fumiko (1903–26), an anarchist who tore up the imperial reprieve that commuted her death sentence, only to hang herself three months later in prison with her self-woven hemp rope.

Hane also deals with the *Sekirankai* (the Red Wave Society), an activist group of socialists and communists who emerged in the early 1920s; women who became active in the labor movement that developed both in the fields and factories; and, finally, Yamashiro Tomoe, a Communist active in the 1930s and imprisoned with her husband in 1940. Hane provides us with a fascinating reading from Yamashiro's *Fuki No To* (Bog Rhubarb Shoots), published in 1948, which tells of her prison life—indeed, "a remarkable testimony to the human spirit" (p. 214).

Hane has done a true service to the field of Japanese studies with this contribution, for certainly one's understanding of prewar Japan is all the deeper after a reading of his work. The title of the book is somewhat misleading, for only one of the women in this study was executed. But my major wish was for a closer examination of what was happening with such rebel women in the immediate prewar and war years. Yamashiro is an exception here, but one might have hoped for a chapter or two in illustration of Hane's two important generalizations about this particular period: "Many of the feminist reformers were in prison or remained silent or even joined patriotic women's organizations to support the war effort." And "many prominent feminist reformers rallied to the nationalistic cause and lent their names to the efforts of the government leaders to marshal national support for their nationalistic, imperialistic efforts" (pp. 27–28). These were significant phenomena in many respects, and portraits representative of these patterns would have rounded off this already fine volume.

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SHERRY B. ORTNER. *High Religion: A Cultural and Political History of Sherpa Buddhism*. (Princeton Studies in Culture/Power/History.) Princeton: Princeton University Press. 1989. Pp. xxi, 245. Cloth \$35.00, paper \$12.95.

Sherry B. Ortner's primary goal is to tell the story of how, by whom, and "especially why" (p. 3) the first celibate Buddhist monasteries were founded among the Sherpas of northeast Nepal. The answer to the

last question leads her into discussions of the relationship between politics and religion and to more general considerations of how people can simultaneously be both products and producers of their culture. Her book is an extensive historical and ethnographic foray into "practice theory," an intricate amalgam built, in Ortner's version, out of four elements: practice, structure, actor, and history. Ultimately, practice theory asks the biggest questions social science can raise: why a given society has a particular form at a particular moment, and how the "people whose very selves are part of that social form nonetheless sometimes transform themselves and their society" (p. 193). In answering the many big and little questions that practice theory poses, her book is meticulous in its detail, persuasive in its narrative, incisive in its analysis, and compelling in its conclusions. Powerfully argued and gracefully executed, it represents a quantum leap forward in our knowledge of Sherpa history, religion, and social structure.

Culturally Tibetan and politically Nepalese, the Sherpas have long been Buddhist, but only in the early twentieth century did they begin to establish celibate monasteries. The short answer to "why" this innovative development occurred lies in the complicated interplay between external factors, including the availability of wage labor, and internal ones, including structural contradictions such as that between egalitarianism and hierarchy. It is unnecessary and foolish, Ortner's analysis suggests, to seek the answer exclusively in either one of these sets of factors.

Establishment of monasteries followed a "cultural schema," a standardized plot structure reappearing in stories and rituals, that showed individual Sherpas ("big people" as well as "small people," both of them key elements in her tale) how to resolve the structural contradictions that surrounded them in ways that generated both personal satisfaction and social respect. The schema consists of five stages: rivalry and competition; departure of the hero and acquisition of a protector; defeat of the rival and acquisition of his subjects; departure of the loser; founding of a temple by the hero. Since even the loser can go on to success elsewhere, there seems to be something for everyone.

Deftly interlacing political economy with symbolic analysis, Ortner's study is neither exclusively "causal" and scientific nor solely interpretive and humanistic. Rather, she blends both these approaches through the mediation of the "intentionality" of individual actors. Her lucid discussion of all of this is conceptually coherent without being theoretically pretentious and concept mongering.

Ortner has an extraordinary ability to cast the whole enterprise in the "once upon a time" style in which a grandmother (admittedly sophisticated and high-powered) would tell her grandchildren a bedtime story, and a delightful one at that. The story is sometimes repetitive, but thankfully so, given the almost Russian novel-like cast of characters!

Most of the scenario she constructs refers to events so remote that one cannot help wondering if this is really how it happened. Ortner offers plenty to quibble over: the overdrawn (in my view) influence of nineteenth-century taxes and their collectors (*pembu*), the insufficiently drawn Solu/Khumbu contrast, the speculative population figures, the importance of potatoes and the existence of indigenous tubers, the overemphasis on the "Rana squeeze," the relevance to fraternal rivalries of the equal inheritance rule. That others can challenge her painstakingly documented case is a tribute to the wealth of detail that she provides.

Ortner has written a provocative, stunning, brilliant tour de force. This pathbreaking work will be of great importance to scholars working on various fronts, among them Buddhist, Nepal, and Tibet studies, folklore, practice theory, anthropology and history, and anthropology of religion.

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N. L. ZHUKOVSKAIA. *Kategorii i simbolika traditsionnoi kul'tury mongolov* [Categories and Symbolics of Traditional Mongol Culture]. Moscow: Nauka. 1988. Pp. 195. 1 r. 40 k.

This new book, by a well-known Soviet Mongolist, is a hopeful indication that Mongol studies are beginning to move from narrow, specific topics—linguistic, archeological, literary—to a more theoretical level, to embrace broader anthropological, cultural, and historic concerns. The author convincingly shows that traditional Mongol culture represents a well-balanced system, where each custom, ritual, and image must be considered not by itself but in relation to other elements of culture. If such an approach is used, some customs, such as leaving a dead person's body in the steppes to be devoured by animals, are not seen as "barbaric." Instead, that custom represented an idea that there was no borderline between the world of animals and the world of the dead. The dead person had to be helped to discard his or her body, so that the soul could quickly depart the body to be reborn and to return to earth (p. 172).

N. L. Zhukovskaia admits that she was influenced by a book of another Soviet scholar, A. Ia. Gurevich, who published *Kategorii srednevekovoi kul'tury* published in 1972 (an English translation appeared under the title *Medieval Popular Culture: Problems of Belief and Perception* [1988]). Zhukovskaia induces a number of universal categories, which can be discerned within Mongol popular culture. Unlike Gurevich, however, who used only the four categories of space, time, law, and work, Zhukovskaia extends the number of categories to include the calendar, holidays, food, popular beliefs, gift exchange, etiquette, numbers, and colors.

Using her own field experience in Mongolia and a

large body of published materials in Russian, Mongol, and Western languages, Zhukovskaia has compiled an impressive amount of information that shows the importance and meaning of each of the above categories. Thus, the reader learns that the organization of space outside and inside the *yurt* was strictly in accordance with the Mongol concept of cosmic space, where each item was imbued with symbolism and carried a sacral meaning. The author distinguishes three categories of time used by the Mongols: mytho-epic time, historic time (which Zhukovskaia discusses in detail in the chapter "Calendar"), and everyday time. Everyday time was measured by very specific events. For example, "the time to saddle a horse" or "the time to boil milk" represented the shortest segments of time, whereas "the time to move to the summer pastures" measured a longer, seasonal time (pp. 32–34).

In the chapter "Food," Zhukovskaia discusses Mongol eating habits and the rituals and symbols the Mongols associated with food. Among other observations, the author correctly notes that a lack of fish consumption among the Mongols was a function of a traditional diet, which put a premium on meat and milk products. Only during severe famines did the Mongols resort to fishing (p. 78).

In the chapter "How to Preserve Happiness," the author deals with a number of superstitions and customs meant to ensure the preservation of "happiness" within a family or a clan. The sheer number of popular beliefs and magic used for this purpose was astounding. Although Zhukovskaia did not come to this conclusion, it is quite obvious that "happiness" often eluded the average Mongol and that the use of magic was intended to provide more security to the fragile lifestyle of a nomad. A discussion comparing the Mongol notion of "happiness" with the similar idea of *kut* among the Turks would have benefited this chapter.

One will find particularly good discussions in the last three chapters on etiquette, numbers, and colors. The author discusses patterns of social interaction of the Mongols and a great variety of rituals that differ according to the gender, age, and kinship of persons. Finally, we learn the sacred role of such numbers as 2, 3, 4, 5, 8, 9, 12, 13, 37, 60, 73, 81, and 108 and the symbolism attached to the colors white, black, red, blue, and yellow.

Despite the abundance of material, this book is more about "categories" than it is about "symbolism." It is essentially descriptive and could benefit from a stronger interpretive approach. The best chapters are the ones on etiquette, numbers, and colors, where the author spends much more time explaining the symbolic nature of the rituals and their meanings rather than merely describing them. This book could also benefit from a wider use of the materials about the Buriats and the Kalmyks. Although sharing the same common culture with the Mongols, the customs and rituals of the Kalmyks and Buriats evolved differently

because of a different ecological, political, and economic setting. Thus, a discussion of the Kalmyk and Buriat customs could provide a broader context for the traditional Mongol culture. On the whole, however, as one of the few studies attempting to synthesize a wide range of the prescriptive Mongol customs, this book is certainly a big step forward toward a better understanding of traditional Mongol culture.

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KENNETH W. JONES. *The New Cambridge History of India*. Volume 3, part 1, *Socio-Religious Reform Movements in British India*. New York: Cambridge University Press. 1989. Pp. x, 243. \$34.50.

According to the general editor of this volume, the intention of the series is not to present the last word on any topic but rather to be an essential voice in a continuing discourse. Kenneth W. Jones fulfills this intention admirably in his study of religious movements. Although the book's brevity compels condensed treatment of many important movements and the omission of others, Jones has helped, as he puts it in his bibliographical note, in the creation of "a coherent vision of the past" (p. 234). Explication of the social role of Indian religion has been a major concern of Western writers on India since the eighteenth century, but there have been remarkably few attempts to analyze and catalog the varieties of Indian religious experience in modern times. The best known, and the one with which scholars in the field still have "continuing discourse," is J. N. Farquhar's *Modern Religious Movements in India* (1914). Although unrivaled for its encyclopedic coverage, it is informed by a self-confident Protestant liberalism standing in judgment on Indian religions. Jones attempts to present his reading of religious movements in a more value-free framework. Even though his book is less comprehensive than Farquhar's, it has the advantage of being able to draw on the large number of monographic studies that have appeared in recent years.

Jones's study is set in the context of what he calls "the colonial milieu," the years of British political domination in the Indian subcontinent from 1757 to 1947. He tends to emphasize the influence of Western culture on the formation of reform movements, but he insists on the importance of patterns of dissent and reform that were present in Indian religion long before the advent of the West. Out of this meeting of the colonial milieu and indigenous culture, Jones argues that two types of religious movements developed, for which he uses the terms "transitional" and "acculturative." By transitional he means movements that arose out of the Indian tradition of dissent with little influence from the West, whereas the acculturative movements are those that were produced in the colonial milieu and whose leaders were the product of cultural interaction. This distinction is useful for

preliminary analysis, but a criticism of Jones's work is that at times he is fairly arbitrary in assigning movements to one category or another. Almost all of the movements include elements from both the Indian tradition and the colonial milieu. Thus, the Arya Samaj and the Brahmo Samaj, the most influential of the nineteenth-century movements, are identified by Jones as "acculturative," but both can be shown to be deeply rooted in patterns of Indian dissent.

Using his two-fold division, Jones surveys movements in the main geographic regions of India. The chapters on North India, where he has sure command of the data and the languages, are the most original and insightful. Two final chapters give a more synoptic treatment to the interaction of religion and politics in the twentieth century. One of his conclusions here is puzzling. The religious reform movements, he argues, did not openly identify with the nationalist movement because to have done so would have brought them into conflict with the British government, which "would have threatened the destruction of all that they had built" (p. 208). The great nationalist organization, the Indian National Congress, certainly came into conflict, but it was not destroyed. One suspects that if the movements did not support nationalism, it was because their roots were in the indigenous tradition that offered few models for this kind of political involvement. But surely members of movements such as the Arya Samaj were in fact deeply committed to the cause of independence.

Inevitably there will be strong disagreements with a book that covers such a vast range of controversial material, but it will provide a starting place for further exploration of a fascinating and bewildering terrain.

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PAUL R. BRASS. *The New Cambridge History of India*. Volume 4, part 1, *The Politics of India since Independence*. New York: Cambridge University Press. 1990. Pp. 357. \$34.50.

This is one of the thirty volumes planned for *The New Cambridge History of India*. Paul R. Brass, a political scientist, maintains through the three parts of this shrewd analysis that the Indian parliamentary system has entered "a grave systemic crisis" as it has failed to reconcile the conflict between efforts of national elites to create a highly centralized state and the countervailing tendencies toward regionalism, decentralization, and ethnic pluralism.

In part 1, on political change, Brass argues that, until Jawaharlal Nehru's death in 1964, a strong center with well-defined goals was balanced by relatively autonomous state governments led by powerful provincial leaders. After Nehru's death, the Congress party's organization disintegrated during the lengthy

succession struggle that his daughter, Indira Gandhi, ultimately won. During the prime ministerships of Indira and her son, Rajiv Gandhi, the Congress lost electoral control of many of India's state governments, either to regional parties or dissident Congress factions. To stem the growing regionalization, Indira and Rajiv employed authoritarian measures, including suspension of elected state governments and imposition of direct rule from New Delhi, while making demagogic promises to abolish poverty.

Part 2 explores how India's exceptional ethnic diversity and rising civil disorder have slowed national integration. In India, thirty-three languages are spoken by more than one million people. Linguistic minorities have agitated to gain the material advantages that come with official recognition of their languages in educational and administrative institutions. Most conflicts over what languages to adopt for official purposes were resolved or contained, as were demands for the reorganization of state boundaries along linguistic lines. On the other hand, where religious minorities have advanced successionist demands, as in the cases of Sikhs in the Punjab and Muslims in Kashmir, New Delhi has responded with military force and provoked more violence. Independent of Kashmiri separatism, Hindu-Muslim conflict has grown since Nehru's death. Brass argues that this conflict is a consequence of both the rise of militant Hindu organizations and "the entrenchment of an ideology of the secular state which, in its tolerant face, justifies pluralist practices but can also be used to condemn minority demands as a danger to national unity and the integrity of the Indian state" (p. 203). Readers are likely to be disappointed with the brevity of the analysis of resurgent Hinduism, given the recent growth of movements appealing to Hindu pride.

Caste rivalries, tribal grievances, and movements to limit labor migration among Indian states are the other main sources of ethnic conflict. Congress and non-Congress state governments have extended the colonial policy of giving preferential treatment from "scheduled castes" (untouchables) and tribals to the somewhat higher "other backward castes." As political parties compete for the support of previously undermobilized castes, some state governments have reserved places in the bureaucracy and universities for a third or more of the state populations. This preferential treatment has led to a backlash among privileged castes, riots, and court challenges to preferential policies.

The third and final section addresses India's political economy and the persistence of hunger, malnutrition, and unemployment. Between 1950 and 1980, growth of annual per capita income exceeded population growth by a meager 1.3 percent per year (p. 256) and left over 30 percent of the population below the poverty line (pp. 262–69). Brass does not accept explanations for mediocre agricultural performance that assign prime responsibility to social inequalities

and a partnership among the dominant "proprietary" classes, consisting of industrialists, bureaucrats, rich farmers, and opportunistic politicians who have been said to monopolize the benefits of development. Although Brass agrees that corruption is rampant in all but the highest echelons of government bureaucracies and that rich farmers have controlled local government institutions and blocked land redistribution, he argues that the fundamental inhibition to rural development has been the Nehru-Mahalanobis strategy of allocating most resources to heavy industrialization. This strategy has aided urban areas, industrial self-sufficiency, and military power at the expense of agricultural development and rural employment. Without policies stressing rural development, decentralization, pluralism, and less authoritarian approaches, Brass predicts not class warfare, which he stresses has been relatively rare in India (p. 240), but further escalations of ethnic violence, political anomie, and erosion of support for fragile parliamentary institutions.

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RICHARD SISSON and LEO E. ROSE. *War and Secession: Pakistan, India, and the Creation of Bangladesh*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xiii, 338. \$39.95.

This is a detailed study of two interlocking crises in 1971 that produced a civil war in Pakistan, the outbreak of war between India and Pakistan, and the emergence of an independent Bangladesh. The first crisis grew out of the Pakistani elections of 1970. With the capture by the Bengal-based Awami League of the majority of seats for the new Pakistan National Assembly, Pakistani leaders faced the difficult problem of negotiating the transfer of power from the army to a new civilian government that could command the support of popular leaders in both East and West Pakistan. Richard Sisson and Leo E. Rose describe in detail the tortuous negotiations that led finally to the army's decision in March 1971 to take military action to suppress the Awami League and to arrest its leader, Shaikh Mujibur Rahman. The result was the eruption of a Pakistani civil war.

The second crisis reflected the impact of this civil war on relations between Pakistan and India. The civil war in Pakistan created a problem for India, in the form of a large flow of refugees and serious instability along its borders, but also an opportunity. Sisson and Rose's account traces changing strategic thinking about the crisis in India during the summer and fall of 1971, both in the context of relations within South Asia and in light of India's relations with major powers outside. Pakistan's faltering attempts to deal with the crisis are also described, as are the positions in the crisis of the Soviet Union, China, and the United States. The conclusion of the crisis came

with India's decision to invade East Pakistan in late 1971, thus precipitating the break-up of Pakistan and the emergence of Bangladesh.

Sisson and Rose's approach is to weave an extremely detailed (and roughly chronological) narrative of the decision-making process within Pakistan and India throughout these crises. They identify the groups and individuals with important input in the process and trace the objectives that they pursued and the constraints within which they operated. They are sensitive to the historical "culture of distrust" between East and West Pakistanis, and between Pakistan and India. They are also sensitive to the popular pressures that operated on many of the key actors in the negotiations, including Shaikh Mujib and the leader of the Pakistan People's party, Zulfikar Ali Bhutto. Having said that, their focus is very firmly set on the arena of high-level decision making and negotiation.

This focus influences strongly the interpretations of events that they develop. For Sisson and Rose, the primary explanation for the violent conclusions of these crises lies in the failures of the negotiating process itself. The contending parties were, in the final analysis, unable to solve these crises peacefully because of the mistrust, misperceptions, and fears (compounded by weak control over their own followers) that shaped key negotiators. The wars that resulted are thus presented as wars that nobody wanted but that happened as a result of the failure of negotiation.

Such a perspective is by no means wholly convincing. The authors themselves present considerable evidence to suggest that some key actors may never have been interested in negotiated compromise at all, except entirely on their own terms. This is possibly the case for Bhutto during the first crisis, and it is virtually admitted by the authors with respect to the role of India during most of the second crisis. To see the wars as wars no one wanted and an outcome largely of the failure of the negotiating process is thus true only in a very narrow sense.

But the heavy explanatory emphasis on the failures of the negotiating process is in large part inherent in the book's firm focus on this political arena. And this is also the book's greatest strength. Sisson and Rose's work is a careful, insightful, and fully researched account of the decision-making process that shaped two of the most important crises in the modern history of South Asia. In this, it is not likely to be soon surpassed.

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DIANE LANGMORE. *Missionary Lives: Papua, 1874–1914*. (Pacific Islands Monograph Series, number 6.) Honolulu: University of Hawaii Press. 1989. Pp. xxiv, 408. \$35.00.

Many years ago I interviewed an elderly French priest in the Solomon Islands. Frail and bird-like, he sat in the glow of the kerosene lamp and described how he had sailed from Marseilles more than sixty years before. He had, he observed, lied only once; when he told his mother that he would see her on his furlough, knowing full well that he had committed himself on behalf of Rome to a lifetime in the islands.

What, I wondered, had motivated him to become a missionary and what had sustained him all of those years? These and other questions are sympathetically and skillfully addressed in Diane Langmore's brilliant group portrait of 327 European missionaries who lived and worked in the neighboring colonial territory of Papua (in southeast New Guinea) between 1874 and 1914. Those missionaries represented four different missions—the Sacred Heart mission, the Anglican mission, the Australasian Wesleyan Methodist mission, and the London Missionary Society (LMS)—that covered the broad spectrum of activity in the golden age of missions of the Roman Catholics, the Anglo-Catholics, the liberal Protestants, and the Evangelicals.

Langmore sets out to demolish or qualify the stereotype of the stern, unbending man of God in his stovepipe hat. Her encyclopedic research (buttressed by highly informative appendixes) reveals that most missionaries (including a remarkably large number of female missionaries and missionary wives) did not fit that mold. Instead, they were solid, dedicated lower-middle-class recruits from Europe, England, or Australia, undistinguished by birth or education, who were reassuringly human in their aspirations, weaknesses, and tolerance. The majority of them appear to have been positively influenced in their religious pursuits by their mothers, an influence reinforced by personal contact with missionaries, exposure to mission literature, and by the mission-mindedness of the communities in which they lived. Some were further motivated by the romantic aura of distant mission labors, the attractions of power and status, or the prospect of splendid martyrdom.

Once in the field, missionaries were obliged to cope with loneliness, monotony, constant ill health, separation, self-doubt, and (in the case of the Protestant missionaries) the painful tensions between religious and familial responsibilities. Conditions were easier for some than for others, and the explanation lay not only in the character of the individual missionaries but in the mission context in which they found themselves. The LMS tended to provide its missionaries with a good deal of support, but parallel emphasis on local autonomy (in keeping with its individualistic conception of Christianity) frequently led to "almost anarchic individualism" (p. 193). The Methodists, by way of comparison, were tightly controlled and unified. These Catholics of the Sacred Heart mission were resolute in their spiritual commitment and sense of community but penniless and hard-pressed compared to the pragmatic Anglicans who

tended to enjoy better living conditions despite weak support from their apathetic and divided church.

Marxist historians have portrayed missionaries as an intrinsic part of colonial exploitation, but Langmore suggests that, although missionaries were indeed deeply implicated in the imperial process, they were, as likely as not, opponents of that same process. At the heart of the matter was a paradox. The missionaries saw themselves as patrons, protecting their Papuan charges from exploitation by ill-informed government officers or unscrupulous traders, whereas they themselves were agents of change as profound as anything visited on the islanders by government or commerce.

This is a splendid book that provides fresh and fascinating insights into the nature of the missionary calling, the missionary way of life, and the place of missionary endeavors in the imperial design. More than anything else, it reveals the danger of viewing missions (or indeed other fraternities) as monolithic bodies. Instead, what we have is "a host of individuals, many of them complex, strong, memorable personalities" (p. 262), seeking to pursue modest, self-denying lives in the service of God.

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UNITED STATES

LEN GOUGEON. *Virtue's Hero: Emerson, Antislavery, and Reform*. Athens: University of Georgia Press. 1990. Pp. xiii, 408. \$45.00.

Slavery represents the most profound moral dilemma with which American culture collectively has had to come to terms. Len Gougeon's book deals with the response of one important northern public figure to the moral and political crisis occasioned by slavery, arguing (in contradistinction to many of Ralph Waldo Emerson's biographers) that Emerson in fact came to adopt a relatively strident posture on the slavery question during the 1840s and 1850s. Drawing on an impressive array of primary documents heretofore untapped by Emerson scholars, Gougeon demonstrates convincingly that "Emerson's thinking on the topics of abolition and social reform in general went through a substantial process of development and change" (p. 20). The events of the 1850s in particular—the Fugitive Slave Law, the Dred Scott decision, the guerrilla warfare of "Bleeding Kansas," and John Brown's raid on Harper's Ferry—propelled him toward the activist orientation of the social reformer and away from the contemplative (and, some would argue, passively acquiescent) social attitudes of his earlier days.

Primarily a scholar of literature, Gougeon frames his study as a corrective to portraits of Emerson that take as his final word on the subject his early condemnation of abolitionists as "men of one idea and women of one idea" (*Journals and Miscellaneous Notebooks*

[1969], 7: 30). He accepts it as a given that the development of Emerson's thinking is worthy of such close attention because of his canonical stature. As the title of the book implies, Emerson comes across as heroic and at times even takes on the characteristics of a Christ figure (as when Gougeon says of his lukewarm commitment to the cause of antislavery in the 1830s that "his time had not yet come" [p. 40]). This is a larger than life man whose relentless struggling with moral questions Gougeon obviously admires.

At the end of the book, even readers who are far less likely to accept unquestioningly Emerson's overarching cultural importance are likely to admire him as well. If his moral dilemmas in the face of slavery did not necessarily exemplify a transcendent standard for the age, they did point up the problems that the new secular public intellectuals, accustomed to dealing with morality in the abstract rather than with human misery in the concrete, confronted as the slavery issue hit closer to home. The outlines of this story ought not particularly to surprise. Emerson began by perceiving slavery as a significant but relatively remote moral issue. Gradually, he was drawn as an active participant into the maelstrom of abolitionism as he learned more about the real human injustices of slavery through research in the 1840s. Events of the 1850s increasingly called into question in his mind whether individuals in the United States could any longer legally follow the dictates of their own consciences. By the time of the Civil War, he had come to see the United States government as an institution dominated by politically expedient men who were actively impeding the moral progress of human civilization. Given his perception that the ability of individuals to act morally was in danger, his full identification in the end with the cause of abolitionism proved fundamentally of a piece with his earlier thought.

Gougeon's exquisite detailing of the ways in which Emerson's moral decision-making process evolved over time yields unexpected pleasures. This is a world that in many ways seems singularly contemporary, and Emerson is a representative man agonizingly struggling with the nuances of moral action in a morally ambiguous world. At what point must one stop relying on education and attitude change and turn to direct political action to achieve moral results? What is the citizen's duty in a democracy where professional politicians seem to be motivated mainly by an interest in perpetuating their own careers? What are the moral trade-offs involved in single-issue politics? Neither Gougeon nor Emerson provide any final answers. Yet this glimpse into another time and place is not designed to do so. Rather, it helps us to see more clearly that even if the question of slavery is no longer immediately with us, the larger question abolitionism raised—how individuals can best pro-

mote justice in a complex and interdependent world—still very much is.

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Oxford, Ohio

GEORGE COTKIN. *William James, Public Philosopher*. (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 1990. Pp. xii, 218. \$32.50.

In this challenging study, George Cotkin contributes to a burgeoning academic industry that has produced at least a dozen books and an even greater number of scholarly articles on William James during the past decade alone. Whereas most have focused on his personal life, his professional activities, and his technical writings, Cotkin presents him as a "public philosopher" who not only addressed public issues but applied to them insights derived from his professional work. More important, these public issues influenced ("inscribed," in Cotkin's occasionally jargon-prone prose) his technical writings.

Reviewing the major phases of James's life from a contextual perspective, Cotkin argues that his non-service in the Civil War was equally, if not more, important than his well-known vocational dispute with his father. Like others in the "generation of 1840," James faced a bewildering choice of career options. For many of his contemporaries the result was endless postponement, neurasthenic illness, and finally palpable relief when the Civil War provided a way out. Denied this relief when he failed to serve, James feared "being judged not to have lived" (p. 29), a fear that sent him to the Amazon with Louis Agassiz in the mid-1860s (an "act of transference") and later inspired the "Will to Believe" (1897). A Harvard Hamlet, James escaped his own debilitating doubts with his promotion to professor of psychology in 1885 and the publication of *The Principles of Psychology* (1890). Addressing the *tedium vitae* that plagued his contemporaries, he then contributed to the continual "discourse of heroism" that blossomed in the 1890s.

Cotkin concludes in his two best chapters that America's imperialistic adventures launched a new phase of James's career. Whereas some have viewed James's anti-imperialism and increased political activity in the mid-1890s as a naive and ineffectual respite between spates of serious writing, Cotkin argues that imperialism forced him to confront still unresolved antimonies between freedom and necessity, individual and group, and self-assertion and lassitude. James's engagement with these public issues, in turn, produced the attacks on abstraction and certitude in his later writings. *Pragmatism* (1907), no less than his more popular essays, expressed his anti-imperialism. To the rationalist, James's pluralistic metaphysics would appear to be "a tramp and vagrant world" (p. 157). His freedom-filled universe, like freedom for

the Philippines, would likewise seem to be "a trunk without a tag, a dog without a collar" (p. 157).

Making this case, Cotkin joins David Hollinger, James Kloppenberg, and others who have stressed James's commitment to morality, intellectual responsibility, and social democracy. He also gently criticizes others who too exclusively stress personal, familial, and pathological factors in the early years, as well as those who argue that James's statements on public issues were, at best, flawed by his professional commitments, or, at worst, a reflection of the imperialist and reactionary spirit he ostensibly opposed.

Cotkin is careful not to claim too much. An emphasis on the Civil War merely supplements familial analysis (p. 20). The "politics of pragmatism" may have been "vague and unimpressive," but James was politically active (pp. 172–73). His personal stake in heroism "sometimes overwhelmed his good judgement" (p. 176). However commendable, this judiciousness leaves one wondering finally how much weight to assign James's "public philosophy" in explicating and evaluating his technical work.

Public concerns in "A Moral Equivalent to War" (1910) and other popular essays come as little surprise. Whether scattered references to "vagrant worlds" (the single such one in *Pragmatism*, as it turns out) add up to either "inscription" or justification is another matter. Despite these questions, Cotkin provides a gracefully written and consistently intelligent defense of James and pragmatism that deserves a wide audience among intellectual historians and their students.

ROBERT C. BANNISTER
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KENNETH CMIEL. *Democratic Eloquence: The Fight over Popular Speech in Nineteenth-Century America*. New York: William Morrow. 1990. Pp. 351. \$24.95.

This book is a valuable account of several American battles about the language appropriate to democracy's leaders and citizens. It records the steady lament over the decline of language, life, and sociomoral distinctions in an integrative society, where the styles that the elite recommended as tests of true worth were both easily mocked and quickly emulated by any who saw value in them. Kenneth Cmiel expresses sensible wariness about much of this linguistic lamentation but essentially organizes the material around his own jeremiad. "Traditions of civic rhetoric virtually disappeared at the beginning of the twentieth century," because the triumphs of academic philology, the plain style, and colloquial speech "helped sap the will for informed public debate" (pp. 16, 262). Well, gosh, constant irritation is the price of others' liberties, but. . .

The book is best where most focused. Cmiel recounts well struggles over biblical revision, both before the Civil War and again in the 1880s, especially

how the varied motives of "reformers" undercut effectiveness. The dictionary controversy in the antebellum years and the radical revisions near century's end are richly detailed, although one could quarrel with Cmiel's conclusion that, in the first round, "Webster failed with the public" (p. 82). Certainly the shifts in the successful Merriam-Webster dictionary were modest, but they, along with Noah Webster's vigorous nationalist and anti-elitist argument, sanctioned the winning idea that dictionaries did not pass on immutable truth but were bound to encase the living language.

Cmiel analyzes surprisingly scantily both the "democratic eloquence" of the title and the "popular speech" of the subtitle. Three pages sensibly explore Lincoln's presidential rhetoric, but there is no other example to suggest from what we have declined, or why we have declined, because Cmiel sees Lincoln's strong use of the *sermo humilis* style repeated in Martin Luther King, Jr.'s oratory. No other political or pulpit speech, congressional or legal debate, or lyceum lecture is explored. John Randolph, Charles Grandison Finney, and John C. Calhoun go unmentioned; William Jennings Bryan appears only as an editor and Daniel Webster merely as someone who sacrificed his high style on the altar of Whig victory in 1840. Cmiel claims that the *Columbian Orator* "transformed" Frederick Douglass, "giving him the voice he had not had," but does not consider how he used that voice (p. 91). Stephen Douglas, we learn, built his career "around bitter personal harangues," but no mention is made of anything he said, including the Lincoln-Douglas debates (p. 63). To use a Douglas phrase, the South in this book is "nowhere." Popular speech is similarly absent except for some slang (including racial-ethnic epithets), noted as entering the late dictionaries. Cmiel neglects all vitally colloquial American literature from Franklin to Thoreau to Twain, including both southwestern humor and local color fiction. He never mentions elementary education where in fact most Americans honed their common tongue and learned their rich common literary culture.

The villains of Cmiel's piece are not better explored. Henry Ward Beecher and Horace Greeley represent religious and political declension, but we hear more about others' attacks on them than their voices, especially unfortunate in Greeley's case, because he spoke tellingly if colloquially about issues in ways that shame his respectable detractors. Cmiel's example of the decadence of recent political language, Teddy Roosevelt, gets a bit more space, but his speeches, allegedly awash in "mom, pie and the flag," get no more convincing attention (p. 250). The other villains are the democratic citizenry, whose at best "half-refined" tastes pulled standards down early, and the linguists of the late nineteenth century, who Cmiel thinks destroyed civic rhetoric by accepting technical, spoken, and colloquial speech, by suggesting that truth in scholarship was more important than uplifting moral tone, and by encouraging what had

been taught under rhetoric in universities to be segregated into literary, linguistic, and composition studies. He might have added speech and drama.

To have an intelligent and richly researched résumé of elite worries and arguments about language, its proper nature and social role, is most welcome. The disappointment is that Cmiel too little explores language's use in favor of reiterating the elites' "simple poles of refined/colloquial, written/spoken, rhetoric/journalism" in terms of "high/popular tensions" (pp. 254, 205). Language is linked to moral life but in ways much richer than the dichotomous tradition that Cmiel gets from both his sources and Theodor Adorno. To segregate the educated from the "half-educated" in terms of words is to confuse status with worth and to neglect how hard it was to be half-educated in the nineteenth century and how impossible it is today.

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DAVID R. ROEDIGER and PHILIP S. FONER. *Our Own Time: A History of American Labor and the Working Day*. (Contributions in Labor Studies, number 23.) New York: Greenwood. 1989. Pp. xii, 380. Cloth \$39.95, paper \$19.95.

In the 1830s, journeyman shipwrights, joiners, caulkers, and mast makers at the Kramp shipyards in Philadelphia looked to establish rules that would regulate their work "reasonably and more consistent with the Constitution of Man." In a printed broadside, Kramp shipyard workers announced their intention to enforce a nine-hour workday "by the ringing of the Mechanics Union Bell." The efforts of shipyard workers to wrest control over their labor illustrate what David R. Roediger and Philip S. Foner identify in this book as the essential features of the movement for shorter hours during its most dynamic periods of organization. According to Roediger and Foner, the tendency to foster unity across lines of craft, gender, and ethnicity; to involve workers in political campaigns as well as trade union battles; and to invoke the class issue of who controls workers' lives on and off the job set the shorter hours movement apart from labor's economic struggles for greater compensation.

Many of the early leaders of the movement for shorter hours concentrated on modifying the working day as a necessary precondition for broader reform of the industrial order. In the 1820s, for example, the Philadelphia artisan William Heighston held that workers' hopes for "meliorating" their condition hinged on self-education, which could be achieved only through the establishment of the ten-hour workday. Similarly, Ira Steward, a Boston machinist and "father" of the eight-hour movement in the middle decades of the nineteenth century, viewed

winning a shorter workday as an initial step but one that was required if workers were ever to achieve their political and economic independence. Despite some victories, the labor movement's success in securing the eight-hour day in the nineteenth century proved evanescent.

Wary of claims for corporate hegemony in the early twentieth century, Roediger and Foner chronicle a persistent resistance movement—highlighted by workers' struggles over the length of the workday—against employers' efforts to impose policies based on Frederick Winslow Taylor's principles of scientific management. In their account, opposition to independent political organization and general mistrust of government action as well as craft organization explain why most American Federation of Labor unions remained aloof from the battles for a shorter workday. Although the authors describe workers' continued agitation over hours up to the present, by the 1920s, they maintain, the class character of earlier efforts had been replaced by a simple desire for greater time off.

Roediger and Foner have produced an encyclopedic account of the movement for shorter hours. While the book's comprehensiveness is its strength, herein also lies its weakness. Rather than offer critical commentary, the authors too often are satisfied simply to catalog workers' heroic struggles over shorter hours. More important, one might assume from reading this study that organizations such as the Knights of Labor, which did not make the shorter workday their key demand, were marginal to the labor movement. Despite these shortcomings, Roediger and Foner have written an extremely valuable account of labor's many battles for justice and liberation, which also serves as a necessary reminder that this war has yet to be won.

BRIAN GREENBERG
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ALICE KESSLER-HARRIS. *A Woman's Wage: Historical Meanings and Social Consequences*. (The Blazer Lectures, 1988.) Lexington: University Press of Kentucky. 1990. Pp. xii, 168. \$19.00.

Alice Kessler-Harris offers "provocative illustrations" of the proposition that wages rest on "a set of social constructs . . . that convey messages about the nature of the world and about fairness and justice within it" (pp. 5, 2). In five suggestive essays, she shows how the new attentiveness to language and discourse can put a fresh construction on old debates. Discounting market mechanisms as determinants of the wage, she denaturalizes the pay packet and shows its contingent, gendered nature.

The first two essays show how policy debates about women's wages through the 1920s were entwined with notions about separate spheres. In "The Wage Conceived," Kessler-Harris argues that women's wages, unlike men's, rested on conceptions of wom-

en's needs rather than the worth of their work. Caught between employers' desires to pay "customary" rock-bottom wages and reformers' assumptions of women's dependence, women wage earners found their labor devalued, their morality impugned, and their ambitions for self-support thwarted. Early minimum wage commissions allowed women only bare survival; during the 1920s they included more consumption in women's needs but failed to assure their self-sufficiency. In a surprising reversal of precedent, the Supreme Court's *Adkins v. Children's Hospital* (1923) overturned these socially conservative minimum wage laws. The Court, Kessler-Harris argues, hesitated between two competing bodies of received wisdom—ideas about gender difference justifying the minimum wage and ideas about freedom of contract condemning it—and opted for the latter by "simply defin[ing] sexual difference out of existence" (p. 53).

The third and fourth essays explore the emergence of the idea of a provider. Kessler-Harris maintains that popular conceptions about women's wage earning during the Great Depression reflected "not . . . a single necessary [gender] dichotomy, but . . . a set of intersecting circles of experience that together structure consciousness" (p. 67). Letters to the federal government, she argues, show that ordinary Americans combined work and family considerations in demanding that jobs be reserved for providers—who might be men or women, supporting their families or themselves—as a matter of social justice. An essay on equal pay shows that the slogan "equal pay for equal work" rested on a central ambiguity: it simultaneously replaced the notion of a needs-based wage for women with one based on justice and promised to protect men's family wage from low-wage female competition. In the end, the second tendency prevailed, linking equal pay to family consumption and national prosperity and accepting the continuation of occupational segregation and thus gender inequality (p. 100). Women had been fully admitted to the category of providers, even if as auxiliaries.

The final essay places comparable worth within a long series of historically specific demands for equality but argues that it breaks new ground by labeling gender boundaries as socially contingent and demanding "nothing less than the revaluation of women" (p. 125). Kessler-Harris explicitly links history and public policy here, cautiously expressing hope that history can vanquish social myths that marginalize women's aspirations as personal choices. Her earlier arguments, however, warn us that campaigns for gender equality such as suffrage and job analysis often had unintended effects that operated against women; as we struggle to write a socially responsible history, we need to challenge consistently attempts to misuse our arguments to undermine instead of foster justice.

SUSAN PORTER BENSON
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CLAUDIA GOLDIN. *Understanding the Gender Gap: An Economic History of American Women*. (NBER Series on Long-Term Factors in Economic Development.) New York: Oxford University Press. 1990. Pp. xviii, 287. \$29.95.

This book is an analysis of women's work force participation and earnings. It is a study in historical perspective rather than an economic history of women as the title suggests. Claudia Goldin uses conventional economists' categories of labor supply and demand factors and human capital to account for changes in the wage gap between men and women over time. The work focuses particularly on the period from 1890 to the present, highlighting the changing significance of marital status for women's labor market work as well as differential rates of participation for nonwhite and white women.

Goldin argues that, although manufacturing showed a very high level of sex segregation and wage disparity, industrialization and the change from agricultural employment to manufacturing in the early nineteenth century did not result in "wage discrimination" (a term always used with quotation marks by the author), which only emerged between 1890 and 1940. She uncritically accepts the assumption that male-intensive industries required higher levels of "physical capital" and skill than others that hired women only; the wage gap can therefore be explained by "productive attributes" such as training, experience, and, especially, greater strength of men relative to women. By contrast, social reformers of that time contended persuasively that women's wages did not reflect some kind of "work worth" but rather women's overall lesser bargaining power and greater vulnerability to exploitation. The author does not adequately address the issue that the entire wage structure was shaped differently for men and women and that men's greater strength was therefore not the determining "human capital" asset in many manufacturing occupations as she assumes. With the expansion of women in clerical work, however, which required a higher level of "human capital" in education, women should have had an advantage over men. It is here that the author recognizes the emergence of "wage discrimination"; although men and women started out with similar skills, the earnings gap between them increased with experience because women were barred from clerical jobs with promotional potential (p. 112). Goldin makes interesting use of cohort analysis, which illuminates life cycle effects, to argue that part of the explosion of married women working in the 1950s and 1960s can be traced to their experience as younger, single women around 1920. The "marriage bar" (policies prohibiting married women from working) and lack of part-time work before the 1950s, along with social norms that a working wife reflected badly on her husband's ability as a breadwinner, served to defer re-entrance into the work force until the 1950s. This increase of married

women's work force participation (for both nonwhite and white women, despite black women's overall higher rates) combined with younger women's increased participation then accounts for the current dramatic increase in working women.

Goldin explores the relative contribution of occupational segregation to the wage gender gap after other factors such as experience, education, and hours of work are taken into account. Because she works within a "human capital" framework, however, the causal direction of certain factors is assumed. For example, one might argue with her assumption that increased individual levels of schooling led to the tremendous expansion of clerical work in the 1920s (p. 145). Goldin's data sources use the National Archives survey schedules, including U.S. Commissioner of Labor reports on working women, Women's Bureau surveys, and other contemporary surveys of women's work force participation. Noneconomists might find her narrowly quantitative supply-demand framework tough going and unnecessarily limiting; this is not for the reader interested in a broad discussion of the interplay of a variety of social and economic forces. The strengths of this work are in pointing out the cohort factors that produce long-range changes over a broad time period.

SUSAN LEHRER

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MARY P. RYAN. *Women in Public: Between Banners and Ballots, 1825-1880*. (Johns Hopkins Symposia in Comparative History, number 15.) Baltimore: Johns Hopkins University Press. 1990. Pp. xii, 202. \$22.95.

Historians have argued that nineteenth-century American women moved only gradually into the public world from the private domain, creating new social structures in order to carry out their roles as republican mothers and moral guardians of family and community. Without specifying how we should now understand women's voluntary associations and their efforts to propound domestic values and promote middle-class culture, Mary P. Ryan recasts the period as having been characterized by a vital and diverse public culture in which women long played a significant physical and symbolic role. Taking the city as her setting, she discusses four aspects of this culture: civic ceremonies, gender geography, and female subjects of public policy, and shapers of public opinion in New York, San Francisco, and New Orleans.

She begins with civic rituals of the 1820s in which gender was expressed through the use of female allegorical figures representing abstract principles and portrayed by relatives of prominent men. By 1840, women had become the prescribed audience for public ceremonies, thereby shaping the narrative

and form of these events. The female body retained its symbolic importance, standing for the newly feminized civic virtues of chastity, sobriety, and industriousness. In parades and festivals, female imagery represented the kinship ties that underscored ethnic and religious identity, thus reinforcing the legitimate claims to public participation by an expanding democracy while venting growing ethnic and class tensions among the citizenry.

The gender geography of the city also changed in 1840. Women had mingled informally in urban space. In a symbolic effort to order the social diversity of the city, women were classified into the "endangered" and the "dangerous." Despite efforts to segregate the dangerous and preserve the sensibilities of the endangered, both groups appropriated public space, politicized it, and acted collectively from it, in mutual support and conflict. Ryan offers the regulation of sexual commerce as a case study of the limits of women's public power, the innovative pursuit of female goals, and the salience of female sexuality as an issue offering parties, governments, and bureaucrats an opportunity to compete for political advantage.

Women expanded their activism with the Civil War, confronting draft officials and military forces in the streets and later taking their concerns to workers' parties, legislative sessions, state constitutional conventions, and women's rights meetings. Using the language of female virtue, they manipulated political symbols, lobbied, rioted, and obtained public funding for their personal charities. Symbolically, gender became more attached to class and racial distinctions, reinforcing the hierarchical ordering of social difference. Ironically, gender also served to unify some groups as elite women transformed the public calendar, giving official sanction and social prominence to their once private celebrations.

As she demonstrates the extent of women's public presence and role, Ryan also reveals the underside of gender politics—the use of women and the female body as ornaments, dependents, and pawns. She upholds women's agency in historical change while begging some causal questions. One could wish for sharper connections between the symbolic use of gender and sexuality in politics and the actions and consciousness of women themselves. Nevertheless, Ryan's elegant essays sketch a chronology of changing gender symbology and contribute to our understanding of the cultural construction of boundaries between public and private. Historians and feminists will pursue for some time her questions about the process and consequences of excluding women from the public arena and their striving for participation in it.

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LORI D. GINZBERG. *Women and the Work of Benevolence: Morality, Politics, and Class in the Nineteenth-Century United States*. (Yale Historical Publications.) New Haven: Yale University Press. 1990. Pp. xii, 230. \$25.00.

In this study, Lori D. Ginzberg places a wide variety of middle-class women reformers—benevolent workers, moral reformers, temperance advocates, suffragists, and charity organizers—in the context of changing class relations and political structures over the course of the nineteenth century. Building on the work of Anne Boylan and Nancy Hewitt, Ginzberg offers a carefully nuanced interpretation of antebellum women reformers. In two especially fine chapters, she argues that the antebellum equation of benevolence with female morality sheltered benevolent women's considerable political influence and shielded their widespread involvement in businesslike activities, offering examples of women's use of corporate organization forms that, as she points out, "contradict everything we thought we knew about the legal status of married women in the antebellum era" (p. 50). To account for her findings, she tempers the familiar interpretation of women reformers as proto-feminists with the caution that reform rhetoric also allowed for the emergence of an antebellum middle class that obscured its own class privilege by wrapping itself in the mantle of benevolent virtue.

As Ginzberg demonstrates, the major challenge to women reformers was the increasing significance of the vote in nineteenth-century American politics. Whereas influential benevolent women tried to ignore electoral politics and female abolitionists proclaimed moral example more important than political practicality, reformers who came of age in the 1850s took partisan politics for granted. During the Civil War, women reformers espoused a rhetoric of nationalism, discipline, and efficiency that laid the basis for a postwar moral reform rhetoric pruned of gender imagery and rooted in a newly defensive middle-class desire to exert social control.

This book is elegantly written and full of insight. Ginzberg's research in biographical and organizational sources allows her to point out significant similarities between benevolent women and men. Her focus on antebellum political restructuring offers a new explanation of the emergence of demands for woman suffrage. Particularly impressive is the way she carries her analysis of gender and class formation into the much-neglected decade of the 1850s.

The strengths of the book far outweigh its weaknesses, but weaknesses there are. Ginzberg underestimates both the subordination of women in the antebellum period and the appeal of the rhetoric of female morality in post-Civil War America. For the late nineteenth-century period, she would have done better to rely less on David Pivar's now outdated work (*Purity Crusade* [1973]) and more on Linda Gordon's insightful critique of social control interpretations (*Heroes of Their Own Lives* [1988]). Particularly curious

is the lack of any reference to the pathbreaking work of Paula Baker ("The Domestication of Politics: Women and American Political Society, 1780–1920," *AHR* 89 [1984]: 620–47).

Although antebellum specialists will find this book more persuasive than late nineteenth-century specialists, Ginzberg's determination to juxtapose issues usually studied in isolation could stand as a model for American social historians of any period. Her questions about the intersections of gender, morality, class, and politics will remain significant for years to come.

PEGGY PASCOE
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KATHLEEN BARRY. *Susan B. Anthony: A Biography of a Singular Feminist*. New York: New York University Press. 1988. Pp. xi, 426. \$27.95.

The neglect of Susan B. Anthony in recent historiography is eloquent testimony to the distaste among today's feminist intellectuals for political history. Not only has there been no biography of the suffragist leader since Alma Lutz's 1959 volume, but Anthony has been all but ignored in the journal literature as well. Kathleen Barry, a sociologist and feminist theorist, has attempted to fill this gap by constructing a life of Anthony that will find enthusiastic readers among a young generation seeking to find in Anthony a feminist after our own hearts. Unfortunately, Barry's readers, unless sufficiently inspired to turn to the primary sources, will come away without the understanding of nineteenth-century conditions and culture that would enable them to place Anthony in her own time and evaluate the choices she made.

Barry is at her best in dramatic re-creations of some of the emotional high points of Anthony's long career: the early and isolated struggles as a temperance, abolitionist, and woman's rights lecturer; her daring attempt to vote and her subsequent arrest and trial in 1872; the vicissitudes of her cross-country campaigns and excitement of her European travels; and the great conventions and jubilees that crowned her fame as venerable director of a united suffrage movement after 1890. The book's most affecting passages touch on the complex and magnificently productive political and personal partnership of Anthony and Elizabeth Cady Stanton, a relationship full of humor and exasperation, abiding affection, and the clash of temperaments and tactics. Anthony's devotion to her own family and to the adopted "nieces," younger activists who became her trusted lieutenants and heirs in the suffrage movement, is also vividly, sometimes gushingly, described.

Impatient with the details of political maneuvering, Barry consistently oversimplifies or neglects the context in which Anthony struggled—a context which both nurtured and resisted her genius as a political broker. Important coworkers such as Clara Colby, the

Clay sisters of Kentucky, Hannah Solomon of Chicago, Abigail Scott Duniway of Oregon, and Lillie Devereux Blake of New York go unmentioned. And other key figures—Rachel Foster Avery, Olympia Brown, May Wright Sewall, Caroline Severance, Alice Stone Blackwell—are reduced to the role of spear carriers. The omission of Colby, who as editor of the National Woman Suffrage Association's newspaper, the *Woman's Tribune*, acted as Anthony's political mouthpiece for many years, is especially puzzling but may be explained in part by the astonishing fact that the *Tribune* does not appear among Barry's sources.

Anthony's tourist adventures during her sojourns in Britain are recounted at some length, but Barry has consulted no British sources and thus leaves Anthony's contacts with British feminists—Millicent Fawcett, Lydia Becker, Lady Aberdeen, and many others—unexamined. One result of this dwarfing of Anthony's colleagues is to obscure the regional intricacies of American suffrage history. Another is to leave crucial political decisions, such as the choice of Carrie Chapman Catt as Anthony's successor, entirely unexplained.

Barry's lack of interest in theological matters and denominational history leaves her ill equipped to illuminate some of the most central motivations and relationships among suffrage workers. Her account of the deep division, not only in the suffrage movement but also between Stanton and Anthony personally, occasioned by Stanton's bitter critiques of orthodox religion is sketchy and often confusing. Barry gives readers no account of the liberal religious movements that nurtured both women, of Anthony's decades-long involvement in her local Unitarian church, or of the denominational affiliations of such prominent figures as Methodists Frances Willard and Anna Howard Shaw or Universalist minister Olympia Brown.

In a brief epilogue Barry summarizes some of the theoretical desiderata for and offers some methodological approaches to the writing of women's biographies from a feminist perspective. She has not succeeded in implementing her own program. But in her passionate account of how she immersed herself in Anthony's writings and other papers, and of the resulting imaginative identification she came to feel with Anthony's personality and life choices, she may give a clue to the ultimately disappointing product of her research. Janet Malcolm, in her recent pitiless dissection of the ethical contradictions of journalism, writes, "Journalists who swallow the subject's account whole and publish it are not journalists but publicists" (*The Journalist and the Murderer* [1990], p. 144). The same might be said of biographers.

GAIL MALMGREEN
New York University Library

JOEL J. OROSZ. *Curators and Culture: The Museum Movement in America, 1740–1870*. (History of American

Science and Technology Series.) Tuscaloosa: University of Alabama Press. 1990. Pp. xii, 304. \$34.95.

Museums have an enormous impact on how Americans think about science, art, and history. Yet little has been written that places the organization and operation of museums in historical perspective. Joel J. Orosz seeks to fill the gap with a narrative of the development of the museum in America before 1870.

Based on profiles of eleven influential museums, including the Peale Museum in Philadelphia, Western Museum in Cincinnati, Barnum's American Museum in New York City, and the Smithsonian Institution in Washington, D.C., Orosz describes the competing claims of popular entertainment and elite scholarship that led to the "American compromise": museums would not be the exclusive domain of either scholarly research or popular amusement. By 1870, the role of the museum as public educator was well established, combining the egalitarian impulse that had led to the founding of lyceums and public schools and the professionalizing impulse that had led to the founding of elite scientific and scholarly organizations.

Orosz's portrait of the museum straddling education and amusement seems plausible, although his evidence suggests that these tendencies existed simultaneously, did not unfold according to the linear progression implied in his tightly organized chronology of chapters, and did not culminate in a happy compromise and stable synthesis. Indeed, the history of museums since 1870 seems to yield the same oscillation between elite leadership and popular appeal as before 1870. The scheme also seems too simple; for example, the grass-roots fundraising campaign to preserve Mount Vernon as a museum in the 1850s cannot be categorized as professional, yet it was not really a popular movement either, as Orosz would claim.

Orosz looks closely at the administrative battles within the institutions he studies, but he has little to say about the content and impact of the exhibits. He offers a tantalizing description of the Western Museum's exhibition of a trip to the "Infernal Regions" in 1828, complete with a wax Lucifer with moving eyes and other special effects, but he then dismisses the display as "exploitative entertainment" rather than analyzing the source of its appeal. Based on what these museums put on display, what did Americans in the nineteenth century consider science? What did they consider entertainment? Or art? Or history? What sorts of items were customarily excluded from display? The connection of museums to culture lies not only in the ways in which museums are administered but also in the categories of classification they present. Analysis of the classification schemes the museums employed would potentially have yielded far richer insights into antebellum American culture than merely that of the conflict between democracy and elitism.

Orosz's narrow focus, for example, leads him to

miss the potential connection between the emergence of museums and mass consumption in the nineteenth century—how the museums' display of objects implicitly connected the public good with goods (commodities). Was the role of the modern museum as popular merchandiser as well as educator, identified by Neil Harris in his essay comparing postbellum museums and department stores, present earlier?

Overall, this story tells far more about the curators than about the culture. Its detailed narrative of conflicts in the organization and administration of museums leaves readers wanting to know more about what these museums were like and the role they played in the lives of their visitors.

DAVID GLASSBERG
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BRIAN W. DIPPPIE. *Catlin and His Contemporaries: The Politics of Patronage*. Lincoln: University of Nebraska Press. 1990. Pp. xix, 553. \$50.00.

This densely packed study is not about art or American Indians as one might infer from a book about the artist George Catlin and his Indian paintings. Rather, it is about the machinations of a number of artists, writers, and antiquarians eager for government support and ruthless in their exploitation of friends, foes, and subjects to obtain it. It is not a pretty story, but it certainly is a fascinating one, and Brian W. Dippie has told it with humor, objectivity, and also a modicum of sympathy for the players in it.

The career of George Catlin (1796–1872) brackets the story, providing its beginning and end. Catlin's wanderlust impelled him westward from Philadelphia to the Rocky Mountains and, later, north to Russian Alaska and south to the Amazon and Peru. The few years he spent in Indian country in the American West in the 1830s, however, were the most crucial. By the time he returned to the East and determined to try his luck in England and Europe, he had completed almost all of the sketches and portraits of American Indians that were to constitute his life's work. After this, he was forced to concentrate on marketing the work; marketing art in the mid-nineteenth century inevitably brought him to Congress's doorstep. Bound to fail in his quest for government patronage given the sectional and constitutional tensions of the period and the personal nature of party politics, Catlin turned to theatrics to support himself. From the grandiose mission of instructing Americans and Europeans alike in the culture and history of American Indians—a race fast disappearing, it was believed, before the advance of Western civilization—Catlin became a showman dedicated to amusing and shocking his audiences with popular lectures and live Indian performances. Always in debt, he was forced to mortgage his collection and never actually recovered it. In an odd way, this was fortunate for his

reputation if not for his well-being: the collection remained almost completely intact and came as a gift to the Smithsonian Institution after Congress had refused to vote funds for its acquisition by that institution.

During his thirty-one years abroad, Catlin's competitors—men who had, ironically, found in him a role model—took over center stage, each intent on receiving government support for his work—literary, scientific, or artistic—on the American Indian. Henry R. Schoolcraft, "Indian Historian to Congress," did receive congressional support for a series of volumes that could be considered a swindle if it were not for its important illustrations by Captain Seth Eastman, U.S. Army officer and artist. John R. Bartlett, antiquarian, Ephraim George Squier, ethnologist, and John Mix Stanley, artist, also sought government patronage at the expense of the others. In their maneuverings to obtain a piece of what was really a very small pie, they established and destroyed reputations, while creating representations of themselves as dedicated artists and scientists and of the doomed Indians, whom presumably they were defending, that entered into the country's mythology and continued to influence American attitudes toward Indian life and history.

Other interesting characters emerge in Dippie's panoramic study: Joseph Henry of the Smithsonian, Albert Gallatin, Brantz Mayer, Charles F. Hoffman, Lewis Cass, Thomas L. McKenney, Paul Kane, and George Winter, to name a few. Because these individuals were constantly interacting as well as pursuing their selfish interests, their story becomes complex, but Dippie has succeeded in so moving them around—isolating and then bringing them together—that their individual motives emerge clearly. His brilliant organization, meticulous research, and human understanding take this book beyond its original purpose. Like all really successful historical studies, it illuminates an entire period and the people who were responsible for it.

LILLIAN B. MILLER
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Smithsonian Institution

JOHN DUFFY. *The Sanitarians: A History of American Public Health*. Urbana: University of Illinois Press. 1990. Pp. 330. \$32.50.

This study is the first large-scale survey of American public health to appear since George Rosen published *A History of Public Health* in 1958. Surprisingly, with the exception of some excellent studies of individual cities and states, the "new" social history of medicine of the 1970s and 1980s by-passed the public health movement, despite its obvious importance both to the development of modern medicine and to the well-being of the American people. But the last few years have seen a revival of interest in the topic, to which this new synthesis will surely contribute.

John Duffy combines his own encyclopedic knowledge with the work of other scholars to produce a sweeping historical narrative of the public health movement. Although he provides competent reviews of both colonial public health efforts and post-World War II developments, the heart of Duffy's narrative, as his title suggests, is the critical century from 1830 to 1930. The nineteenth-century public health advocates known as sanitarians blended piety and science to create a new ethic of civic cleanliness. Duffy traces the rise of the sanitary movement from its first efforts to clean up the urban environment in the mid-1800s through the subsequent growth of city, state, and federal initiatives in the Progressive period.

One of the virtues of Duffy's survey is his broad conception of public health, which he defines as "community action to avoid disease and other threats to the health and welfare of individuals and the community at large" (p. 1). He does not confine his attention to the specialty of public health and its often uneasy relations with the rest of the medical profession, although that story forms an integral thread in his narrative. Duffy places that theme in the context of other efforts by nonmedical groups to improve the public health, including public health nursing, voluntary health associations, private foundations, and child-health advocates.

Another virtue of Duffy's synthesis is his attention to regional variation. His previous research on public health in New York City and Louisiana is augmented with an exhaustive coast-to-coast survey of local and state histories. Using these sources, Duffy is able to outline significant regional differences in the organization and development of public health services.

Although appreciative of public health efforts in the past, Duffy reviews their achievements with a critical but fair eye. He gently decries the tendency to separate the medical and social aspects of disease as public health professionalized, and he never loses sight of the pivotal role of poverty in creating ill health. This book is not a Whig vision of constant upward progress: Duffy's portrayal of the fragmentation of today's public health services makes the decades from 1880 to 1930 seem even more like the "golden age" of American public health.

Given the sweep of his narrative, it is inevitable that Duffy is forced to overgeneralize on occasion. For example, historians familiar with European radical movements in the nineteenth century will be jarred by his characterization of the British, French, and German people as "more docile and obedient to authority" than Americans (p. 138). Duffy sometimes succumbs to presentist judgments; for example, he describes the late nineteenth-century interest in meteorology as far less "productive" than the collection of vital statistics and the improvement of sanitation (p. 152).

But these are minor flaws in an otherwise magisterial account of the American public health movement. Duffy is to be commended for the impressive

job of research and interpretation that he has done in this volume. It will be of great value to future generations of historians interested in the development of the American public health movement.

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JOHN W. SERVOS. *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America*. Princeton: Princeton University Press. 1990. Pp. xxiii, 402. \$49.95.

American experimental and theoretical science was second-rate compared to European science in the nineteenth century, but, by the second decade of this century, American science had pulled abreast of its European competitors (and parents) in a number of fields. One was physical chemistry, which, by the outbreak of World War I, had given rise to research institutions strongly competitive with those in Europe and by the 1930s had produced one of the greatest of all physical chemists, Linus Pauling. In this brilliant account, John W. Servos sets about to chronicle how and why this came about.

Physical chemistry was itself a very new field at the turn of the century; the decade of its "formation" was the 1880s, with the development of the ionic theory of electrolytic solutions. In his opening chapter, Servos delineates masterfully the goals and methods of this new field: the founding chemists and their disciples turned to physics (particularly thermodynamics) and mathematics as well as experimentation to generate what Wilhelm Ostwald (who became the institutional leader) hoped would be an "allgemeine Chemie" focusing on reaction processes.

Ostwald trained a generation of chemists in the new field at his institute at Leipzig. Many were Americans; they returned to their native land to spread the new gospel. The remainder of the book describes how they developed and institutionalized physical chemistry. At the time of their return, the demand for chemistry teaching at colleges and universities was increasing rapidly; the generalist perspective of physical chemists made them good bets in the classroom, even if their employers were hazy about what this new field was. A few had ambitious visions of establishing research centers; one, Alfred Amos Noyes, succeeded magnificently, first at the Massachusetts Institute of Technology (MIT) and then again in the 1920s at Caltech.

Noyes is the hero and principal focus of the book. He had the "right" vision for research: mathematically and physically based chemistry that developed the original focus on solutions. In his greatest MIT student, G. N. Lewis, this research program led to major breakthroughs in the theory of atomic structure and chemical bonding; his even greater Caltech student, Pauling, provided a yet more impressive

sequel by bringing into synthesis x-ray crystallographical analysis and the new quantum mechanics of Erwin Schrodinger to produce what is still the governing theory of chemical bonding.

Servos uses Wilder D. Bancroft of Cornell as a dramatic counterfoil to Noyes. Scion of a distinguished family, Bancroft chose the "wrong" research vision for physical chemistry: qualitative, antiphysicist (except for the "phase rule"), focused on complex systems. Although energetic and productive in his own way, Bancroft's reputation was to descend even to disrepute by the 1930s.

In many ways, Servo's approach is traditional: there are no epistemic qualms over scientific quality or advance; the sociocultural perspective is more limited than that, for example, of Daniel Kevles's *The Physicist*, if perhaps more subtly deployed. These traits, however, have the virtue of enabling Servos to focus in great depth on the development of research schools and research programs in American physical chemistry. Moreover, in his account, he brilliantly interweaves the scientific component—the actual research programs—with the social and institutional ones. The history of both modern chemistry and modern American science are much the richer for the publication of this book.

SEYMOUR H. MAUSKOPF
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MICHAEL WILLIAMS. *Americans and Their Forests: A Historical Geography*. (Studies in Environment and History.) New York: Cambridge University Press. 1989. Pp. xxii, 599. \$49.50.

Americans, the forester Bernhard Fernow said in 1889, had to learn to treat the forest "as a crop rather than a mine or quarry from which we take what is useful and then abandon it as waste" (p. 403). The shift from "forest as mine" to "forest as crop" was indeed an enormous and surprising change. As Michael Williams shows in this study, Euro-American colonization was at its core an "assault" on trees. For over three centuries, trees fell for a variety of causes: to open land for farming, to construct and to heat buildings, to build ships and to provide them with masts, tar, pitch, and turpentine, to supply charcoal, potash, and fuel for engines. Beginning in the mid-nineteenth century, alarms began to be heard, with "timber famine" becoming a more and more likely prospect. Forest use modeled on mining could not go on forever.

In a synthesis requiring considerable courage, Williams covers this whole vast story: Indian use of forests; the cutting and girdling of trees on the Atlantic Coast and the eastern interior; the industrial boom in logging the Great Lakes states; the South's acquisition of the status of "the epitome of industrial capitalism in the lumber industry" (p. 263); the big leap westward to the Pacific Northwest. As is the

nature of synthesis, certain subjects will strike some readers as neglected; reading this book in Colorado, for instance, does call the reviewer's attention to the fact that the forests of the Rocky Mountains get short shrift. But, noting omissions, the reader also notes valuable, and sometimes unexpected, inclusions. Williams looks closely at the New Deal shelterbelt program as a historically revealing attempt to help the Great Plains recover from their "deficiency" of trees. His descriptions of the various devices for the transporting of logs are vivid and instructive, and his characterization of the social consequences of industrial logging gains from an element of human compassion added to historical analysis. Particularly valuable in this book is the attention paid to the initial government forest officials, Franklin Hough, Nathaniel Eggleston, and Fernow—people who thought seriously about forest depletion but who (in part, thanks to their successor, Gifford Pinchot) are more often characterized as men devoted to the writing of ineffective speeches.

Williams draws his material from the writings of travelers, naturalists, and government officials, as well as historians. Given the enormous size of his topic, his decision to confine himself to printed sources is perfectly justified. Indeed, this work is proof that these sources, printed or not, are stocked with fresh ideas and unrecognized insights. The book is, moreover, plentifully supplied with helpful maps, tables, and pictures.

The most thought-provoking aspect of this study is its cheerfulness. In recent times, Williams notes, "the story of destruction had changed to one of regrowth and birth" (p. 4). He is encouraged, as well, by the emergence of "cooperation" between the federal government and the forest industry. The story of Americans and their forests seems to end, if not happily, at least satisfactorily. And yet current conflicts over forest cutting, in both the Pacific Northwest and the Rockies, call this sanguine appraisal into question. Fernow's hope that forest use would shift, in approach, from mining to farming seems to have been realized. But, as Williams notes, an articulate group of Americans have come to find farming just as inadequate a model as mining; to the eyes of preservationists, a managed and replanted forest is no substitute for a stand of uncut, old-growth timber. Social and political conflict over forest management seems, on the whole, to have increased in this century; if an era of consensus and cooperation is on the horizon, that horizon remains a distant one. "[T]he moving on is nearly finished and the grabbing must stop" (p. 409), Rudyard Kipling said of his American tour in 1889; a century later, the definition of what constitutes "grabbing" in matters of forest use is itself still very much up for grabs.

PATRICIA NELSON LIMERICK
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THOMAS R. COX. *The Park Builders: A History of State Parks in the Pacific Northwest*. Seattle: University of Washington Press. 1988. Pp. xiii, 248. Cloth \$35.00, paper \$14.95.

Thomas Cox traces the development of the state parks systems of Idaho, Oregon, and Washington from the Progressive era through the 1980s. He also discusses related conservation activities: the preservation of roadside environments (timber, wildflowers, and shrubs), regulation of highway billboards, and efforts to save Oregon's Willamette River system, to name several. As the title implies, Cox contends that individuals played key roles in the formative years of the region's conservation movement. In Oregon, newspaper editor Robert W. Sawyer was inspired by a visit in 1921 from Stephen Mather, director of the National Park Service, promoting park-to-park scenic highways from the Columbia Gorge on the Oregon-Washington boundary to California. Sawyer eventually enlisted the support of Governor Ben Olcott, Senator Charles McNary, and others in the crusade to preserve the state's scenery. But the man who deserves the most credit for Oregon's parks system was Samuel Boardman, appointed in 1929 the first parks engineer (the title was later changed to superintendent). An able administrator and an articulate preservationist, Boardman served until his retirement in 1950. During his tenure, Oregon's parks evolved from only the scenic to embrace scientific and historic sites as well. The parks movement in the state of Washington often paralleled Oregon's with leadership provided by Robert Moran, Herbert Evison, Asahel Curtis (the noted professional photographer and nature lover), and various governors and bureaucrats. Although Idaho's Heyburn State Park was the first established in the Pacific Northwest in 1911, the Gem State did not have a parks system until Governor Robert E. Smylie's administration in the 1950s. Before World War II, Idahoans associated conservation with the "locking up" of valuable resources and federal interference with states' rights. As a result, Idaho has had more in common with its politically conservative neighbors, the Rocky Mountain states, than with Oregon or Washington.

This study has been a labor of love for Cox over the last three decades, and he knows his subject well. His research is thorough: printed materials, papers, and records of participants, government agencies, and special interest groups; personal interviews; and monographic literature. The author's bibliographic essay covers published and archival sources and affords a succinct historiography of the state parks movement. Furthermore, Cox grounds the evolution of the Pacific Northwest's state parks in its national context. His treatment of Progressivism, for example, offers convincing evidence that the movement remained strong in Oregon and Washington during the 1920s.

Unfortunately, as sometimes happens with case

studies of local topics, one bogs down in repetitious details of political infighting over obscure bills and appointments important to the respective parks but of little interest to anyone else. But without these particulars, Cox could not have written the perceptive syntheses in the introduction ("The Northwest and the Nation: A Parks Movement in the Making") and conclusion ("Parks and Their Builders in Perspective") that mark the book as a work of mature scholarship and a model for subsequent regional studies.

JOHN JAMESON
Kent State University

ROBERT LOWRY CLINTON. *Marbury v. Madison and Judicial Review*. Lawrence: University Press of Kansas. 1989. Pp. xii, 332. \$35.00.

J. M. SOSIN. *The Aristocracy of the Long Robe: The Origins of Judicial Review in America*. (Contributions in Legal Studies, number 52.) New York: Greenwood. 1989. Pp. x, 359. \$45.00.

Inevitably the bicentennial of the U.S. Constitution has begun to bear fruit in a new crop of "judicial review" books. These two publications suggest that the harvest can be very thought provoking or very aggravating.

Robert Lowry Clinton's book, although it exhibits some weakness in historical bibliography, is more profound and more analytical than J. M. Sosin's. Taking Chief Justice John Marshall's *Marbury* decision as the point of departure, Clinton argues that Marshall simply refused to take jurisdiction contrary to the provisions of the federal Constitution. Limited to the facts, *Marbury* was a case dealing with judicial authority. Although it dealt with a congressional act (the Judiciary Act of 1789), it did not assert the Supreme Court's authority to review congressional acts in all situations, nor did it create the doctrine of judicial review. Judicial review, in fact, did not come into vogue until well after the Civil War. Even then the use of the term was rare until the centennial year of the *Marbury* decision, when the organized bench and bar divided into separate camps to debate the newly named doctrine. From that early twentieth-century debate came the myth of *Marbury* and the conviction that Marshall was dealing with judicial review in 1803. This is a persuasive argument, well constructed and thoroughly researched.

Perhaps Clinton has gone too far in contrasting antebellum constitutionalism with the constitutionalism that followed the Civil War. The jurisprudential impact of the antislavery movement, documented in 1974 by William E. Nelson (*Harvard Law Review*, LXXXVII, 513-66, at 530), and the significance of post-Civil War constitutional thought are well established. Less studied is the receptivity of Marshall and his associates (particularly Joseph Story) to concepts of natural, or "higher," law. The fact that "higher"

law ideas flit through antebellum Supreme Court decisions suggests that Marshall and his contemporaries viewed *Marbury* in broader terms than those suggested by Clinton. On the other hand, it is quite true that none of them would have gone so far as to identify the U.S. Supreme Court as the only definitive interpreter of constitutionality. For modern Americans, *Marbury* has evolved far beyond Marshall's most expansive thoughts, and Clinton provides an excellent description of the creation of that myth.

Unfortunately, Clinton neglects certain circumstances that should give pause to his restrictive interpretation of *Marbury*. In Anglo-American law, and particularly under American concepts of separation of powers, it is the judiciary that has the final say concerning governmental action and individual rights. There is no practical difference between a court declaring a statutory provision unconstitutional and simply refusing to apply the statute to the case before it. The decision remains binding in such a case and would probably have precedential impact on the next court that considers a similar matter. One need not call a judge's reasoning judicial review, nor need a court make an express declaration of unconstitutionality before a statute is rendered useless.

Whereas Clinton stimulates thought, Sosin invites attack. Sosin disclaims expertise as a legal historian or a lawyer (p. 4) and devotes the rest of the book to proving his limitations. Because he covers, in a critical way, the history of law and the legal profession from the time of Henry II to the aftermath of *Marbury*, he claims ample scope for error. Even a historian who remains untainted by the aristocratic long robe of the law should have done better. Essentially Sosin views judicial review as usurpation by a legal profession determined to gain the whip hand over the rest of society. We have long heard the Progressive era historians' lament about judicial review's undemocratic impact on American economic and political life, and we do not need still another book on the topic, especially such a weak one.

Writing on colonial constitutional practice, Sosin asserts that the Privy Council did not exercise judicial review of colonial legislation—it was not judicial review because no English judges participated (pp. 140-41); he subsequently asserts that judicial review is not an appropriate term for the review of a subordinate legislature's acts (pp. 145, 146). The numerous state constitutional decisions that are frequently cited as embryonic forms of judicial review in the Confederation period are dismissed as cases in which judges refused to apply a statute, or failed to declare it null and void (see p. 215; see also p. 265). The author asserts that there is some distinction between "judicial control" and "judicial review" (pp. 230-31), which seems to involve the same faulty reasoning that attenuates the distinction between refusing to follow a statute in a single instance and more broadly declaring it void.

Antebellum judicial review, given the tendency

toward legislative supremacy in the early republic, was rarely exercised and frequently criticized. But it was firmly anchored in traditions of limited government, written constitutions, imperial control, and individual rights. Marshall did not invent the doctrine, but he gave it a practical thrust that was theretofore lacking—the thought that when the judge does his duty and upholds the constitution against conflicting legislation, the citizen is safe from arbitrary government and the nation is protected from legislative excesses. Sosin's research provides ample basis for a sounder contrary interpretation; it is a pity that he has worked so hard to be so wrong.

HERBERT A. JOHNSON
University of South Carolina

ROBERT A. WILLIAMS, JR. *The American Indian in Western Legal Thought: The Discourses of Conquest*. New York: Oxford University Press. 1990. Pp. xi, 352. \$39.95.

This long legal treatise by Robert A. Williams, Jr., is built on four articles that he published in law journals between 1983 and 1989. Williams argues that American treatment of Indians rests on the theory that discovery of lands by European nations gave them superior rights over indigenous peoples and that this "discourse of conquest assert[ed] the West's lawful power to impose its vision of truth on non-Western peoples through a racist, colonizing rule of law" (p. 325). The results have been not only "unquestioned abrogation and unilateral determination of tribal treaty and property rights" (p. 325) but also a denial of other fundamental human rights. "Violent suppression of Indian religious practices and traditional forms of government," Williams writes, "separation of Indian children from their homes, wholesale spoliation of treaty-guaranteed resources, forced assimilative programs, and involuntary sterilization of Indian women represent but a few of the practical extensions of a racist discourse of conquest that at its core regards tribal peoples as normatively deficient and culturally, politically, and morally inferior" (pp. 325–26). The author asks for the replacement of the doctrine of discovery and its discourse of conquest by "an indigenously articulated New World discourse of peace" (p. 326).

Before readers get to these conclusions, however, they must work their way through a complex account of western European legal views about non-Western peoples, running from the Crusades through humanistic themes of natural law, the investiture controversy, the assertions of papal rights to determine dominium in newfound lands, Protestant formulations of a discourse of conquest, and English colonizing attempts in North America to conflicting theories of how Indian title to land could pass legally to whites. The culmination of the discussion deals with John Marshall's support of the doctrine of discovery in *Johnson v. McIntosh* in 1823.

The book is not easy reading, and the details are not always directed clearly to the conclusions. The author's account is based on secondary works for the most part, a choice he justifies by saying that he sought only to "situate the role of Western law and legal discourse in the West's will to empire in the New World" and that "a moderately reliable narrative framework" was all he needed (p. 7). Williams is enamored of the term "discourse" and its derivative "discursive," so much so that their overuse ultimately becomes an irritant to the reader.

Yet there is much of value in this book. It points out the deep roots of Western legal theory, and it analyzes essential documents, from papal bulls to United States Supreme Court decisions, that enunciated it. For readers unacquainted with the standard secondary works on which he relies, Williams's discussion will be richly informative.

Williams asserts that the European conquest of the New World was a legal enterprise, that law was an appropriate instrument of empire, and that law legitimated and energized European actions (p. 6). Here, I think, is the fundamental weakness of the book. The author offers a useful survey of the principal European opinions about natives of the New World, but he does not show convincingly that legal theory was the principal cause or foundation of the monstrous results that he cites in his conclusion. In concentrating on legal discourse, he neglects other formative influences on American Indian affairs. Indian treaties (which recognized Indian sovereignty and land titles), military subjugation, well-intentioned civilizing and educational efforts, court decisions recognizing Indian rights, and so on, are historical forces that have contributed to the present status of Indians in the United States.

FRANCIS PAUL PRUCHA
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JOSEPH B. HERRING. *The Enduring Indians of Kansas: A Century and a Half of Acculturation*. Lawrence: University Press of Kansas. 1990. Pp. xii, 236. \$25.00.

Joseph B. Herring presents an engaging account of displaced Indian peoples' struggles to maintain their respective identities in eastern Kansas. The author focuses on the general events covered by Craig H. Miner and William E. Unrau in *The End of Indian Kansas: A Study of Cultural Revolution, 1854–1871* (1978). Herring's perspective varies significantly from that of Miner and Unrau's earlier work. His study reveals Indian communities striving against enormous odds to preserve major elements of culture against encroachment from hordes of non-Indians who surrounded them and appropriated the greater parts of their lands.

The author points out the difference between acculturation and assimilation in cross-cultural contacts. He ably argues that the Potawatomis, Kickapoos,

Chippewas, Munsees, Iowas, and Sacs accepted many trappings of Euro-American culture while successfully resisting losing their sense of self and community.

Herring's volume traces the fortunes of these peoples through their removal to Kansas during the 1820s through the 1840s. Separate chapters deal with events concerning Vermillion Kickapoos, Chippewas and Munsees, Iowas and Missouri Sacs, Mokohoko's "Trespasser Band" of Sacs, and also Prairie Potawatomis.

Writing of Vermillion Kickapoos, Herring examines the pivotal role of Kenekuk, the Kickapoo prophet whose syncretic religious teachings combined elements of traditional native religious belief and practice with selected parts of Christian doctrine. Herring states, "Unified by Kenekuk's religion and teachings, they had never forgotten their tribal heritage, and their acceptance of white culture stopped short of assimilation" (p. 54).

The author traces varying strategies of cultural survival employed by bands selected for focus in his work. Those portions of the book dealing with resistance to allotment of tribally owned lands are especially enlightening. An eighth and final chapter, aptly titled "The Triumph of Indian Kansas," examines circumstances of eastern Kansas Indian communities in the twentieth century.

Herring has exhaustively researched his work. He draws on a wide variety of sources, including primary source material from national and other archival sources. He has also used virtually every secondary book and article available on the topic. Herring's book is at its best in portraying Indian peoples of eastern Kansas as persons who exercised remarkable power of choice in determining their own fates. Too often Indian peoples are depicted as powerless victims, unable to influence events shaping their lives.

The work is slightly marred by frequently unsupported assumptions of attitude, thought, and emotional state, for example, "Mokohoko realized that violence was not a feasible option for small numbers of Indians completely surrounded by white settlements" (p. 103). These are storytellers' artifices and, if kept in mind, add greatly to the readability of Herring's work.

Herring's book has a copious bibliography and some well-chosen illustrations. It will be useful to specialists on Indian history for its sharply defined community studies and for the overview of allocation and assimilation policies at work preceding the Civil War. General readers will find that the book will catch and hold their interest to the extent that they are drawn into the well-written narrative.

D. C. COLE
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CATHERINE L. ALBANESE. *Nature Religion in America: From the Algonkian Indians to the New Age*. Chicago:

University of Chicago Press. 1990. Pp. xvi, 267. \$24.95.

Catherine L. Albanese has given us another useful book. But, unlike Robert Bellah, whose 1967 essay on "civil religion" cut a master key to many doors, she provides a rather unwieldy clutch whose various keys each turn but one latch. Thus, this book opens onto various rooms in American religion's mansion, but we wonder how—or whether—they are connected. There is a delight in such entry—none of which, to the author's credit, is forced—but we remain confused about the floor plan. And we do not leave with a sense that "nature religion" is an indispensable term for students of America's religious history.

The author is aware of the problems in linking so many different thinkers and does not pretend that her study is a *pas-de-partout*. As she puts it, "We come to find that in truth" there were in America "nature religions." Thus, for some people, "nature meant the physical world"; for others, it became "an abstract principle." For some, nature "meant the truly real"; for others, "the emblem of a higher spirit" (p. 12). With such a broad, phenomenological definition of religion, Albanese moves from the Amerindians whom the Europeans encountered on their first landfalls to the Greens and ecofeminists of our own age.

The author constructs the book around scores of case studies that to her epitomize the various incarnations "nature religion" has taken in America. As noted, chapter 1 treats the differing world views of the Amerindians and the American Puritans, the Puritans in greater depth; here Albanese does not provide anything beyond what we could glean in more detail from scholars of the period. The second chapter, "Republican Nature," runs from the early American playwright Royall Tyler, through the Masons, William Bartram, Thomas Jefferson, and on to Davy Crockett, among others, to suggest how the notion of landscape in eighteenth and early nineteenth-century America (recently very ably treated by William Lawson-Peebles in his *Landscape and Written Expression in Revolutionary America* [1988]) could by Andrew Jackson's day eventuate in manifest destiny. Again there are no surprises in her cast of characters or in how they are used. It is not until well into the nineteenth century, beyond her discussion of the Transcendentalists, that Albanese hits her stride in her section "Physical Religion." Herein she treats handfuls of fascinating figures—the herbal healer Samuel Thompson, the dietary reformer Sylvester Graham, various hydropaths, osteopaths, and chiropraths. Such characters, like the Christian Scientists with whom she closes the previous chapter, "The Transcendental Religion and Its Legacies," are indeed an unorthodox and significant bunch. As Albanese puts it, if in this period Americans "were confusing the physicality of nature with metaphysical idealism and if they were equating harmony with control, they were also merging the gospel of grace

with the Goddess of Reason" (p. 123). This is indeed worth pondering.

The last chapter—a whirlwind tour touching down on, among other topics, the religious activity of Sun Bear and his "medicine wheels," the gemlike prose of Annie Dillard, and the Japanese healer Reiki—again reminds the reader that there finally is very little, really, that links the various figures in this study, particularly those of different periods, except that all of them, in some way or other, found in nature some larger import. To Albanese and the editor of the distinguished series of which this book forms a part, this is enough to make the study worthwhile. Furthermore, in our day the book will have currency because many will welcome the author's attempt to construct a history of various beliefs that mark our late twentieth-century spiritual travail.

PHILIP F. GURA
University of North Carolina,
Chapel Hill

FORREST G. WOOD. *The Arrogance of Faith: Christianity and Race in America from the Colonial Era to the Twentieth Century*. New York: Alfred A. Knopf. 1990. Pp. xxii, 517. \$29.95.

Forrest G. Wood, who in 1968 published a book on racist demagoguery during Reconstruction, remains admirably outraged by the evils of racism and thoroughly convinced of its centrality in American history. In this book, he offers an explanation of its source. Christianity, Wood argues, "has been fundamentally racist in its ideology, organization, and practice" (p. xviii). It not only caused American racism but led to slavery. "English North Americans embraced slavery *because they were Christians, not in spite of it*" (p. 38), Wood writes. There "has been no greater religious force in the dehumanization of humans" (p. 12).

Wood attributes Christianity's central role in racism, slavery, and dehumanization to its monotheistic and missionary zeal, its insistence that it is "The Way" (p. 3). He describes biblical defenses of slavery and racism and then shows how colonial and antebellum churches, North and South, supported the racial status quo. Even most Christian abolitionists, Wood adds, worried more about "their own souls than" about the "suffering of four million slaves" (p. 315). Wood must argue that few Christians sincerely sought to prevent the dehumanization of African Americans in order to sustain his argument against Christianity. If some ardent Christians fought for equality or humanity, then that might suggest that something other than Christian beliefs led other believers to racism and slavery. His argument also necessitates that he dismiss the importance of Christianity among the slaves, which several historians contend served to preserve their humanity. The "number of slaves who became Christians," Wood

maintains, "was far smaller than anyone imagined—then or since" (p. 138). Many African Americans thought to be Christians only incorporated Christianity into their traditional religious systems without fully accepting its morality. In a brief closing chapter on developments after 1865, Wood adds immigrant Christians to his list of racists and chastises the Social Gospel for not addressing the issue of race.

Wood develops these arguments from an extensive examination of secondary works and printed primary sources; he makes no use of unpublished letters and diaries or local church records, sources that often reveal nuances of thought about race and religion. Indeed, several historians have employed such records to demonstrate unusual interracial equality during revivals. Their findings undermine Wood's thesis that by its very nature Christianity caused racism and slavery. Wood's own account also does so. He contends, for example, that slavery's defenders misinterpreted the Bible. They should not have needed to misread it; surely the sacred text of a faith that caused racism would provide a consistent and logical defense of it. In other places, Wood's evidence seems to suggest that support for slavery remained a matter of "profit over piety" (p. 288), to use one of his chapter titles that actually evokes not his but older conceptions of the relationship between slavery and Christianity.

Many scholars have argued that Christians embraced and justified slavery and racism; this book reaffirms that conclusion. The originality in Wood's book rests in the contention that Christianity caused racism and slavery—a charge it fails to sustain.

GAINES M. FOSTER
Louisiana State University

REX EUGENE COOPER. *Promises Made to the Fathers: Mormon Covenant Organization*. (Publications in Mormon Studies, number 5.) Salt Lake City: University of Utah Press. 1990. Pp. viii, 235.

Ralph Waldo Emerson helped spawn a hoary tradition when he characterized Mormonism as "an after-clap of Puritanism." Although many historians have used the ancestors of the earliest Latter-day Saints as the starting point of their discussion of the church, fresh connections are still to be made. Rex Eugene Cooper's work on Mormon covenant making is one of the most recent attempts to do so.

Cooper, however, is not as interested in Puritanism as a prologue to Mormonism as much as an analogue. Accordingly, he begins his book with an extended summary of the work on New England's covenanted world by such historians as Perry Miller, Edmund Morgan, and Larzer Ziff. Although he shies from showing a direct connection between the two societies, he reveals a similarity in their basic structure and subsequent fate as internal and external pressures reshaped their world. In the case of the Mormons,

whom he suggests were bound even more closely by covenants than their forebearers, this meant three major shifts in their use of covenants over time. In the 1830s, covenants primarily concerned "the gathering" of the Saints and the church's commitment to economic egalitarianism. In the 1840s, following the Mormon War and the failure of communitarianism in Missouri, Mormon Prophet Joseph Smith refocused covenant making toward creating ritually derived kinship networks. These, too, declined during the Utah period, as the use of covenants gradually became a sacred, private matter between husband and wife in the twentieth century.

Cooper's study is the first extended treatment of Mormon covenant theology, a significant subject, and in that lies its primary value. Nevertheless, there are problems. Even if the Puritan covenanted community faced some of the same basic problems of adaptation as the Mormon community did two centuries later, when used simply as a explanatory model, the Puritan world offers only modest insights into that of their descendants. Whereas an analysis of how the Latter-day Saints reworked New England traditions might have proved of greater use, Cooper explicitly rejects this approach (p. 3). More worrisome is the book's lack of a firm historical grounding. Cooper betrays only a limited knowledge of Mormonism's relationship to American culture. Too often his brief forays into this area result in a broad but superficial pronouncement about such matters as Mormonism's conflict with American individualism. Although he does attempt to explain how the larger society and dynamics within the church altered the covenant system, he makes little effort to explain what compelled church members to make covenants about their various areas of concern in the first place. Equally unfortunate is that a great deal of discussion rests on outdated, biased, or sometimes misapplied historiography.

Nevertheless, Cooper does something important. If his theme lacks a strong historical context, at least he knows well Mormon covenant theology and how it works. Although historians have acknowledged the importance of covenants in Mormonism, Cooper has convincingly shown by the book's end that they have underestimated their significance. Particularly insightful is his explanation of the rise of the church's patriarchal order and its meaning in terms of religious authority, race, marriage, parent-child relationships, and group identity. It is for this discussion that historians of Mormonism will turn to this book.

KENNETH H. WINN
Missouri Historical Society

LESLIE WOODCOCK TENTLER. *Seasons of Grace: A History of the Catholic Archdiocese of Detroit*. Foreword by EDMUND CARDINAL SZOKA. (Great Lakes Books.) Detroit: Wayne State University Press. 1990. Pp. xiv, 598. \$45.00.

Leslie Woodcock Tentler's necessarily huge social history of Catholics in Detroit, from the founding of the frontier diocese in 1888 to its contemporary postindustrial challenges, makes it clear that the dominant register of American Catholic history must be ironic. The work of Catholic immigrants in reconstructing and improvising their religious worlds on these shores, in their encounter with the topographies, demographics, politics, and economics of the United States, on the one hand, and in fierce competition not only with each other but also with their prelates and children, on the other, has made for a history of unintended consequences and unexpected inversions.

Detroit Catholicism, as Tentler tells its story, was most exuberantly democratic and pugnaciously independent when it was most foreign, in the days of immigration; then, as more and more of the city's clergy and laity were born and reared in the United States, the archdiocese became increasingly Rome-centered, rigidly hierarchical, and clerically dominated—became most foreign, in other words, to the idioms of this country. The ironies of American Catholic history were especially evident in the ebullient and confident days of the 1950s, when Catholics prided themselves on their isolation from the society in which they had struggled so hard to succeed and from which they took the standards of their achievement.

Tentler's study is organized around a series of such peculiar contradictory trajectories. In excellent chapters on the emergence of an ethos of "hearty masculine solidarity" among Detroit's priests, she points out that the city's clergy were most estranged from their congregations when they were most like them, because it was easier for the hierarchy in the 1940s and 1950s to weld boys who shared a common American language and culture into a separate (and increasingly isolated) fraternity than it had been for their predecessors at the turn of the century to do anything with the ethnic clergy. Tentler also shows that it was just when Detroit's women religious had finally succeeded in professionalizing their many works (from hospital administration to elementary education) and in securing long-sought independence from the intrusions of male prelates that they began leaving their orders in record numbers.

Tentler's range is wide, and she makes a valuable contribution to the work of extending the reach of church history, which has been underway for some time now among historians of American Catholicism. She describes children's role at funerals, clerical alcoholism and the diocese's changing response to it, the rumored appearances of St. Joseph in the city's streets during the depression, the evolving idioms of popular piety, Catholic contributions to civil rights, and the changing curricula of religious education classes, to cite just some of her interests.

The book closes with a careful account of the ambivalent response of Detroit's white Catholics to

the city's black population. Again Tentler's narrative is inclusive: she discusses the bitter racism of the neighborhoods, the hesitant interventions of the hierarchy in the racial situation, the courageous and difficult work of black Catholic clergy in the city, and the very early efforts of white Catholic activists to shape an interracial Catholic community in Detroit.

Tentler obviously had the cooperation of the diocesan archivists in this project, and one hopes the example of her rich and complex achievement, with its many provocative indications for future research (just what was St. Joseph doing in Detroit anyway?), will inspire other dioceses to undertake similar projects, too, and soon.

ROBERT A. ORSI
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Bloomington

ANDREW R. L. CAYTON and PETER S. ONUF. *The Midwest and the Nation: Rethinking the History of an American Region*. (Midwest History and Culture.) Bloomington: Indiana University Press. 1990. Pp. xix, 169. \$25.00.

This book originated in a manuscript prepared for discussion at a conference at Indiana University in Bloomington marking the bicentennials of the Northwest Ordinance and the U.S. Constitution. Charged with analyzing the "state of scholarship" (p. ix) on the founding document and the subsequent history of the Old Northwest (later referred to by the authors as the Midwest), Andrew R. L. Cayton and Peter S. Onuf provided a symposium of scholars with a provocative and challenging interpretive essay, one that now emerges from a prolonged discussion of its theses as an equally provocative, challenging, and highly significant work. It is both an impressive full and wide-ranging historiographical essay as well as a brief, stimulating overview of midwestern history from the 1780s to the 1920s.

The authors begin and end with references to Frederick Jackson Turner, whom they praise for contributions to regional history, specifically the region that he and they argue merged its own history into that of the nation. But they also differ with Turner—at one point calling his famous thesis "ethnocentric, racist, sexist, dichotomous, or just plain superficial" (p. 126)—and issue a clarion call for a new, larger, more inclusive, and more nuanced interpretation of midwestern and national history. Cayton and Onuf go part of the way toward providing just such a history, weaving their comprehensive reviews of recent works (primarily since 1950) on midwestern politics, society, and culture into a cogent and connected analysis of events over a century and a half. Their story is told from a Federalist-Whig-Republican perspective that also incorporates analyses from new social, political, and labor history and the best of republicanism and ethnocultural scholarship. They claim, first of all, that the Turnerian dichotomy in

frontier society between aristocratic and democratic forces was an imperfect analysis; rather, concerns for republican values and their perpetuation informed the major political and economic policy decisions and led, in time, to the clear domination of commercial capitalism and the values of a white Protestant middle-class society. In separate chapters the authors explore the origins of community within the region, stressing the importance of various but particularly religious "trans-local communities" (p. 48) and the rise of an urban, or village, bourgeoisie; the development of political parties and institutions, most significantly the Republican party after the mid-1850s that represented the dominant values of the new community; and the process by which cultural hegemony was achieved by the dominant group in the late nineteenth century. They conclude with an analysis of the malaise that set in during the 1920s when the nature of the dominant culture had shifted from individual to corporate liberty, and employment had changed from individualized occupations to varying degrees of specialization and routinization within an industrialized economy.

Given the purpose of this volume and the importance of the documentation (fully one-third of the whole), it is unfortunate that the notes are crammed into pages at the end of the book. Cayton and Onuf seem to have read everything of significance related to their inquiries, and the annotated citations alone make a valuable contribution. This is an ambitious work that, despite the triteness of the sentiment, truly belongs on the "must do" reading list of all midwestern and American historians.

RALPH D. GRAY
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Indianapolis

PAUL C. NAGEL. *The Lees of Virginia: Seven Generations of an American Family*. New York: Oxford University Press. 1990. Pp. xiv, 332. \$22.95.

In this study Paul C. Nagel refurbishes the portrait of one famous lineage. Like Burton J. Hendrick, who wrote the first collective biography of the family in 1935, Nagel chronicles the changing fortunes of the first seven generations of Lees, beginning with Richard Lee who emigrated to Virginia in 1639 and built the family's position and wealth. After devoting fifty pages to the first three generations, Nagel spends the remainder of his book on the revolutionary Lees—for example, Richard Henry, Francis Lightfoot, and Henry "Light-Horse Harry" Lee—and their nineteenth-century descendants, most notably Robert E. Lee. Writing primarily for a nonspecialist audience, the author eschews footnotes but includes a brief note on primary sources and secondary works.

Nagel updates earlier sketches of the Lees in several respects. In particular, he draws on modern scholarship about the politics of colonial and revolu-

tionary Virginia to set the careers of the seventeenth- and eighteenth-century Lees in context. Readers who wish a brief introduction to all aspects of Robert E. Lee's life will find Nagel's account instructive and interesting. Unlike Hendrick, who wrote largely about the Lees' public service, Nagel also chronicles marriages, child rearing, and similar family topics. Consistent with the interest in women's roles shown by his earlier work, (*The Adams Women: Abigail and Louisa Adams, Their Sisters and Daughters* [1987]), he pays particular attention to the activities and actions of females who were born or married into the Lee clan. Many readers will appreciate his lively account of Hannah Ludwell Lee Corbin, who flouted both religious and social conventions in the late eighteenth century. Hannah Corbin appears to have been determined to thwart her husband's will, which stipulated that her remarriage would strip her of most of her inheritance. She brought her doctor into her household, joined the Baptist church as he had done, but registered no marriage even though she soon gave birth to one, then another, child.

Nevertheless, this study, despite its broad focus and general readability, is poorly connected to issues in social or cultural history, in particular, to the history of the family. For example, although Nagel gives the provisions of wills drawn up by various members of the Lee family, he never draws on books and articles about inheritance in the Chesapeake, even though these could have refined his interpretation. Moreover, he describes Hannah Ludwell Lee, deceased in 1750 at age forty-nine, as dying "relatively young" (p. 46), a judgment that neither her contemporaries would have reached nor demographic studies would second. Rather than viewing the family as a social institution or set of cultural forms, Nagel adopts an older, more traditional view that indeed some of his subjects might have held. He appears to indicate that "blood will tell," when he stereotypes whole families with comments about the "arrogant, imprudent and impetuous" Lees (p. 6), the Harrisons "famous for strong natures" (p. 40), or the "notorious Ludwell trait of aggressive pride" (p. 87).

Some historians will question Nagel's explanations of behavior, which rely not only on bloodlines but also on timeless personal characteristics that operate without reference to social or cultural milieus. This is evident in his account of how Ann Hill Carter Lee, wife of "Light-Horse Harry" and mother of Robert E. Lee, dealt with personal and financial tragedies. Rather than exploring norms governing options open to nineteenth-century southern elite women, Nagel compares her to the long line of Lee wives and daughters: "And yet no family member responded to an ordeal with more dignity and patience than Ann Lee" (p. 194). While these approaches limit the importance of *The Lees of Virginia* for social and family historians, readers wishing to learn more about the

history of this famous first family of Virginia will want to begin with this book.

JANE TURNER CENSER
George Mason University

JOHN L. BROOKE. *The Heart of the Commonwealth: Society and Political Culture in Worcester County, Massachusetts, 1713-1861*. New York: Cambridge University Press. 1989. Pp. xxi, 446. \$34.50.

All historians of early America will have to reckon with this outstanding study of central Massachusetts. John L. Brooke persuasively argues that, whereas classical republicanism shaped consensus in Worcester County during the colonial period, after 1780 republican ideology slipped to the margins and was replaced by the previously insurgent liberalism that became "the center of a new consensus" (p. xviii). Brooke's tendency to equate Lockean liberalism with "voluntarism" and Harringtonian republicanism with "community" risks flattening both complicated traditions, but to his credit Brooke acknowledges their internal instability as well as the variety of uses to which they were put. Brooke demonstrates how republican and liberal ideas provided competing patterns of meaning for the culture that emerged in central Massachusetts, and he shows how the analysis of text as rhetoric can illuminate social, economic, and political development. This study rests on a remarkable data base, including printed materials and quantitative evidence concerning population, kinship, wealth, occupation, debt, religious affiliation, and various forms of legal and political activity. Brooke skillfully uses these materials to illustrate the interdependence of different sorts of historical inquiry and the undesirability of pursuing any of these questions in isolation from the others. The ideas of autonomy and community, for example, are not disembodied abstractions but the terms that the people of Worcester County used to make sense of their everyday experience. Their political passions cannot be understood apart from their religious commitments, and neither can be reduced to economics or demography.

After an introductory foray into the environmental history of central Massachusetts prior to English settlement, Brooke explores in part 1 the provincial world of prerevolutionary Worcester County. Separate chapters on institutions, economy, religion, and politics somewhat artificially divide the multidimensional reality Brooke wants to present. He succeeds, however, in showing how different regional patterns of economic activity, religious enthusiasm, and political insurgency imperfectly reinforced each other during these years. Part 2 traces these themes through the revolution and the ratification of the constitutions of the Commonwealth and the nation. The Whig gentry that prized order and civic virtue within the covenant tradition found itself challenged

by an increasingly unruly group of insurgents seeking to legitimate dissent and voluntarism. As Brooke points out, the patterns of affiliation were exceedingly complex, and historians' efforts to establish the primacy of ideological, economic, or religious determinants oversimplify the surprising and ironic process whereby republican and liberal ideas gradually reversed positions as orthodoxy and dissent in the debates of the 1780s. In part 3, Brooke shows how party divisions developed from these origins and how a new center of gravity emerged between these northern and southern sections of the county around the rapidly expanding "central corridor" of manufacturing towns. Brooke's evidence indicates that the fissures in Worcester County reflected class lines less than differences in "culture and tradition" (p. 319). Finally, in his epilogue, Brooke concludes that the Republican party succeeded because it consolidated themes from these competing traditions. Whereas economic change rendered the "Harringtonian social landscape . . . more dream than reality" (p. 389), Republican party ideology managed to combine the themes of liberal voluntarism and classical virtue. The strength of that combination depended—as did the ideology of the revolution—on the compatibility, however unstable, of Lockean rights and Harringtonian duties, which was possible when both were conceived within a religious context that circumscribed and sanctified both sets of values.

JAMES T. KLOPPENBERG
Brandeis University

WALTER J. FRASER, JR. *Charleston! Charleston! The History of a Southern City*. Columbia: University of South Carolina Press. 1989. Pp. xiii, 542. \$29.95.

Walter J. Fraser, Jr., offers an encyclopedic history of Charleston, South Carolina, from the first English settlements of 1670 to Hurricane Hugo in 1989. From demography, politics, and economic booms and busts to the city's government, its epidemics, schools, theaters, and "The College," from piracy and Indian wars to the siege of Charleston during the Civil War and the civil rights movement of the 1960s and 1970s, the episodes and conditions of the city's life are registered, often with vivid anecdotes and sharply worded portraits.

The book is designed as a chronological narrative; its framework is supplied largely by political events and its characters dominated by a tiny elite made up of successful merchants and their planter and lawyer allies. Such men stage-managed two revolutions, the first against the colonial proprietors in 1719 and the second against the British; before and after the Civil War they created effective barriers against the democratic and entrepreneurial styles that were developing within most nineteenth-century American cities. Fraser confirms the picture of an anomalous Charleston elite, poorer but unrepentant after 1865 and

willing to give up power within the state for status within the city, maintaining allegiance to the Episcopal church and to aristocratic manners, both arrogant and defensive in its disdain for boosterism and innovation.

Staples for export and a black labor supply not only produced the wealth and structured the economy of the city down to the New Deal but also dictated the city's characteristic social strains. Fraser tells us that there was no middle class in Charleston for most of its history. Black and poor white people provided the counterpoint to the elite. One of the strengths of this volume is the attention devoted to black history. Many previously little-known names are entered into the record from Thomas Jeremiah, free black martyr to white envy and fear in 1775, to Septima Clark, heroine of the civil rights struggle. Still, much of the history that is told remains the story of what was done to slaves and to African Americans after emancipation. A similar reading can be made of the material provided on poor white people in Charleston. Fraser's narrative provides a valuable survey of services and controls imposed on poor whites over time but only rarely gives a clear sense of the concerns of white working people or the distinctions among them.

The broad span of Fraser's history makes it possible for us to appreciate the tenacity of Charleston's elite. Not until the New Deal and in many areas not until after World War II was Charleston's economy transformed by infusions of federal funds and what, to the old establishment, looked like invasions of tourists. Finally, it took the civil rights movement, federal law, and sunbelt migrations to move the old elite from center stage.

Urban history scholars probably will find those monographs that provide a comparative context for Charleston's status more challenging, studies such as William H. Pease and Jane H. Pease's *The Web of Progress: Private Values and Public Styles in Boston and Charleston, 1828–1843* (1985) and Don Doyle's *New Men, New Cities, New South* (1990). Nevertheless, the breadth of coverage and its reliance on recent scholarly literature make this volume a valuable reference tool. Its bibliography, which includes a significant number of unpublished dissertations, will be much appreciated. On the other hand, the logic of its footnoting system seems dubious; footnotes appear to be assigned by the amount of space filled by the text rather than by subject. Some tables, particularly of population figures, would have been useful.

ANITA S. GOODSTEIN
University of the South

JOSEPH F. RISHEL. *Founding Families of Pittsburgh: The Evolution of a Regional Elite, 1760–1910*. Pittsburgh: University of Pittsburgh Press. 1990. Pp. xii, 241. \$34.95.

Since the colonial days, most Americans have wanted to believe that they lived in an open-ended society. In contrast to Europe, where hereditary nobilities and special privileges made it extremely difficult for persons to supersede their origins, Americans have boasted of "careers open to talent" that allowed capable and hard-working citizens, freed from artificial barriers, to ascend the ladder of social and economic success. As a corollary, many have also held that the country's fluid society did not guarantee success to the sons and daughters of well-to-do parents, whose offspring would have to rely on their own skills and ambitions.

Because the existence of social mobility has been an essential part of the American credo, most early historians accepted its truth without question. But in recent decades social historians have undertaken numerous statistical studies in an effort to find out how much social mobility has prevailed at various times and places in the nation's past. Among the most famous of these studies is E. Digby Baltzell's *Philadelphia Gentlemen: The Making of a National Upper Class* (1958). In this book, as well as in *The Protestant Establishment: Aristocracy and Caste in America* (1964), Baltzell asserted that a rigid upper class had developed in the United States by 1900, one that was so impenetrable from below that it resembled a closed caste in many ways.

Joseph F. Rishel examines the question of social mobility among twenty of Pittsburgh's successful families over a period of one hundred and fifty years. Rishel follows Baltzell's multidimensional approach to class and social mobility by studying such factors as occupation, residence, family background, religious affiliations, school ties, and club memberships. But Rishel faults Baltzell for selecting a group of successful descendants in 1940 and then tracing them back to upper-class ancestors a century or more before. For Rishel, that approach was "ahistorical."

A far better study of social mobility, Rishel believes, would begin with a set of early upper-class families and then trace their descendants forward over several generations (in this case from 1760 to 1910) in order to discover how many had maintained their forebears' social position. His sample comprised all known descendants of twenty selected Pittsburgh families, including those who moved away from the Pittsburgh area (Allegheny County) and those who remained. The figures for both upward and downward mobility among family members, Rishel asserts, might offer some indication of how accumulated social and economic advantages have affected the fluidity of American society.

Of the roughly six hundred descendants of the twenty Pittsburgh families for whom occupational data could be found, Rishel discovered that 75 percent of them had achieved success and maintained their positions in the elite upper class. Another 19 percent were only marginally successful, and only 6 percent were failures. The successful descendants,

Rishel concludes, benefited greatly from the social and economic assets of their families, which gave them a distinct advantage over others when starting out in life.

Although Rishel's study is thought provoking, well researched, and well written, a number of other such investigations will be necessary before historians can draw more definite conclusions about social mobility in the nation as a whole. Nevertheless, this book is likely to become a classic in the study of American class and social mobility.

DAVID R. CONTOSTA
Chestnut Hill College

STANLEY NADEL. *Little Germany: Ethnicity, Religion, and Class in New York City, 1845–80*. Urbana: University of Illinois Press. 1990. Pp. viii, 242. \$37.50.

Between 1860 and 1880, New York City contained the third largest German population of any city in the world, behind only Berlin and Vienna. Kleindeutschland (Little Germany), the main German-American neighborhood on the Lower East Side of Manhattan, had in 1875 a larger population than the cities of Detroit and Milwaukee combined.

The growth and maturity of this neighborhood—the largest ethnic community in mid-nineteenth-century America—are traced by Stanley Nadel in this short, clearly written book. To outsiders Little Germany appeared as an undifferentiated "Dutchtown" (p. 4), but Nadel shows that it was in fact divided in several ways. Repeatedly emphasizing the complexity of Kleindeutschland's social order, Nadel focuses on three significant cleavages.

Religious differences weakened the unity of German New York. Protestants, Catholics, Jews, and Freethinkers all had substantial followings. Nadel, however, describes Little Germany as basically secular and suggests that religious diversity had only a limited influence on the community. Far more important were subnational and class divisions.

The German nation was created only in 1871 out of thirty-nine diverse regions and city-states, each with its own history and culture. These subnational variations had a strong influence in Kleindeutschland. There were distinct Bavarian, Prussian, Hessian, and Badener concentrations in Little Germany. Nadel also shows that regional differences influenced Kleindeutschland's families; there was a tendency toward regional endogamy and family structure.

Little Germany was also divided by social class. By examining the occupations of the city's German Americans, Nadel shows that as early as the 1850s a class structure was emerging. Although the middle class of skilled workers and shopkeepers was unusually large, there were also large numbers of semi-skilled workers and a growing German-American elite that led to "fully fledged class antagonisms" (p. 90). By examining trade unions and political activity,

Nadel concludes that by the 1870s a German (not subnational) working-class consciousness had become the predominant force in German New York.

Nadel's intention is not to provide "a complete compendium of all facets of New York's German-American life" but to sketch "out the main lines of Kleindeutschland's social organization" (p. 155). In this he largely succeeds; his succinct treatment gives readers a good idea of what this important ethnic community was like. There is a cost, however. By limiting his focus Nadel fails to link effectively Little Germany to the city as a whole. There is little sense of how this huge ethnic district fitted into a rapidly growing New York City. For example, to speak as Nadel does of "Kleindeutschland's economy" is misleading; there was a New York City economy and many, if not most, German Americans labored outside of Kleindeutschland for bosses who were not German. Although Nadel's approach helps us understand the insularity with which many Kleindeutschland residents perceived their community, it is less effective in showing how the immigrant community related to the city.

RICHARD STOTT
George Washington University

GARY EDWARD POLSTER. *Inside Looking Out: The Cleveland Jewish Orphan Asylum, 1868-1924*. Kent, Ohio: Kent State University Press. 1990. Pp. xiv, 240. \$32.00.

Eighteenth-century America supported orphans by aiding the remaining family or boarding children out. During the nineteenth century, orphan asylums, often founded by religious organizations, took over this task. Early in the next century, asylums faced attacks from reformers and state regulation. By mid-century, federal aid to families largely supplanted the asylums' work.

Gary Edward Polster's book provides an interesting case study of the Cleveland Jewish Orphan Asylum (JOA). Founded in 1868 by the western division (midwest and south) of B'nai B'rith to care for the orphans of Jewish Civil War soldiers, the institution increasingly received children from poor German-Jewish immigrants. Founded and run by well-to-do German Jews, the institution initially sought "to educate, discipline, rehabilitate, and refine lower-class boys and girls" to make them middle class and American (p. 49). By the late 1880s, JOA's population began to reflect the growing numbers of East European Jews who migrated to Cleveland and other midwest industrial cities.

Cleveland's gentile elite attempted to "Americanize" and "civilize" newcomers through settlement houses and public schools, and they were not alone in their efforts. Polster finds that assimilated German Jews feared that the newcomers' strange ways would endanger their hard-won gains and unleash a wave of

anti-Semitism. As with their gentile counterparts, German-Jewish leaders "attempted to Americanize and uplift the morals and values of eastern European immigrant children" (p. 198).

To protect children from "bad" influences, JOA administrators restricted children to the campus and orphanage school, censored mail, and limited visitation with relatives. They regularly demeaned the children's Yiddish culture and religious orthodoxy and strongly promoted their own Reform Judaism instead.

Polster remains ambivalent about the impact of these efforts. Although JOA "failed to prepare the orphans to cope with the problems and stresses of the city," nevertheless "most . . . succeeded . . . because they learned their lessons well at the orphanage." Moreover, as with "most other children of eastern European Jewish immigrants," they "blended into the American mainstream" (pp. 202, 203).

From 1914 on, new superintendents and "the growing ascendancy of eastern European Jews" (p. 202) in the community helped to bring about significant reforms. Nevertheless, JOA leaders continued to support the asylum approach while "the placing-out and adoption system became increasingly popular among many midwestern Jewish communities about 1915" (p. 224).

The study effectively uses institutional records and twenty-nine extensive interviews with JOA graduates as well as their diaries, letters, and written recollections. Fifty-six photographs provide valuable illustrations, but not all are located near the appropriate text.

Ironically, the book's inward institutional focus reflects the asylum's effort to block out the outside world. It omits the battles outside JOA walls between German and East European Jews, including the founding in 1920 of the rival Orthodox Jewish Children's Home. Largely lacking the context of orphan asylums generally, it is difficult to know how typical JOA was or the significance of these findings.

Nevertheless, this is an interesting and valuable case study. Polster opens the asylum's windows enough to permit an important glimpse into its inmates' lives and the intentions of its managers. Kent State University Press deserves special praise for the attractive layout.

JAMES BORCHERT
Cleveland State University

JOHN MORGAN DEDERER. *War in America to 1775: Before Yankee Doodle*. (The American Social Experience Series, number 16.) New York: New York University Press. 1990. Pp. xvii, 323. \$40.00.

The title of this book might give some people a mistaken notion of its contents. This is not a survey of colonial wars on the Eastern Seaboard of North America. Nor is it primarily a study of British colo-

nists' stated views of war before the American revolution. Only chapter 6, "The American Experience," is concerned mainly with what colonists said and did about war. From it we learn that Americans accepted many of the prevailing European standards for army discipline and operations, yet they believed that non-professional soldiers could fight well, and they knew through experience that they could do a great deal of damage.

Most of John Morgan Dederer's text is a panoramic, eclectic survey of what Americans probably knew and thought or could have known and thought about war as a result of reading the books available to them. Part I draws on the work of many scholars who have studied education, literacy, and the dissemination of knowledge in eighteenth-century America. It seeks to establish the essential premise for what follows: the legitimacy of drawing a composite picture of Americans' preconceptions at the time that they went to war to win national independence. This is, then, an exercise in inference. Dederer suggests that colonists read the history of war as they read political history—selectively, with an eye for evidence of the superior strength conferred by love of liberty.

The book's ostensible subject—war in America, or the image of war in America—sometimes gets lost as the reader follows Dederer hastily through the works of many writers ancient and modern. On one especially rich but not altogether atypical page (p. 82), we meet Mao Zedong, Sun Wu Tzu, Herodotus, Thucydides, Napoleon Bonaparte, Carl von Clausewitz, Adam Smith, William Shakespeare, Vegetius, Niccolò Machiavelli, Maurice of Nassau, Marshal de Saxe, and Frederick the Great. The book's speculations and suggestions about colonists' views are plausible, but Dederer seldom distinguishes between early Americans who might have absorbed all of the influences he cites and people who could have known only a few. The plan of his book hardly allows such discrimination, because he only intermittently mentions colonists at all. Oddly enough, the most learned American revolutionaries—Thomas Jefferson, John Adams, or Benjamin Rush—were more handicapped than helped by their erudition when they tried to guide or explain combat.

Dederer's greatest accomplishment is bringing together the diverse strains from which eighteenth-century readers derived their ideas about armies and war—ideas they took to be truisms confirmed by experience. He convincingly contends that they were all the more ready to fight for a republic because they thought they knew how citizens waged just and righteous war.

CHARLES ROYSTER
Louisiana State University

STEVEN M. DWORETZ. *The Unvarnished Doctrine: Locke, Liberalism, and the American Revolution*. Durham, N.C.: Duke University Press. 1990. Pp. x, 247. \$37.50.

Regardless of how sympathetic the reader might be to the basic premise of Steven M. Dworetz's monograph on John Locke and the American revolution, it is hard to find terribly much of value in this essay. The work seeks to show that, contrary to much current historical writing, civic humanist thought played little, if any, role in shaping the ideology underpinning the American founding and that, indeed, the first principles of the revolution bear the unmistakable imprint of Lockean political doctrine.

While a careful examination of the political literature of the period will, I think, bear out the author's principal contention regarding Locke's importance, Dworetz's study is, unfortunately, often flawed by slipshod textual analysis and unwarranted interpretation. For example, he is certainly justified in underscoring the fact that *Cato's Letters*, which has been interpreted by J. G. A. Pocock and others as an archetypal republican work devoid of any important Lockean influence, does indeed bear Lockean elements. But it is difficult to imagine that anyone who has read these letters with any care could overlook the fact that Locke's influence throughout is substantial. Even more curious is Dworetz's conclusion that these letters owe much of their political philosophy neither to Locke nor to the civic humanist tradition but, rather, to the *Leviathan*!

Similarly, Dworetz's insistence on distancing American revolutionary thought from anything that smacks of an ideology grounded in the sanctity of private property requires that he purge Locke of his "bourgeois" attributes. Thus, Dworetz characterizes the claim that property rights (the rights to acquire and retain property) occupy a crucial role in Locke's political theory as an attempt to make of Locke "the corrupt prophet of the spirit of capitalism" (p. 22). Dworetz insists that a careful reading of Locke's work points to a "theistic" Locke who has little if anything in common with a "bourgeois" Locke. The reader is never told, however, how these two interpretations, the bourgeois and the theistic, are incompatible nor how the "bourgeois" Locke (bad) differs from the "liberal" Locke (good). Indeed, to claim that these two aspects of Locke's thought are at bottom irreconcilable is to ignore Locke's identification of the purposes of God with the laws of nature, the chief of which are man's inalienable rights to life, liberty, and estate.

The interpretive weaknesses in this essay are exacerbated by a grating style that is alternately sententious and self-serving. The reader is treated to a succession of comments on the difficulties involved in mastering both history and political science—difficulties that the author, unlike many scholars who have written in this area before him, has successfully overcome. In addition, the monograph contains a number of asides, occasionally quite lengthy, together with a brief final chapter dealing with the author's position on contemporary political issues and with his ideological antipathies. Does it really add much to our understanding of the American founding to learn

that Dworetz finds contemporary libertarianism a philosophy that can find no place for virtue or that modern American conservatism seeks to restore virtue to our society only at the cost of freedom?

In sum, the volume is at best a mixed blessing. It does a service to scholarship by reminding us of the importance played by Lockean theory in shaping the arguments put forward by the American revolutionaries, but it does so in a work written in a provocative style and blemished by an unsophisticated approach, made worse by shoddy analysis and conclusions that often lack textual warrant.

RONALD HAMOWY
University of Alberta

IAN K. STEELE. *Betrayals: Fort William Henry and the "Massacre."* New York: Oxford University Press. 1990. Pp. viii, 250. \$32.50.

Constructed at the head of Lake George in upstate New York in 1755, Fort William Henry lay athwart the natural invasion route running north-south from Canada across Lake Champlain to the Hudson River. In 1757 it was besieged by French regulars, *troupes de la marine* (Frenchmen trained and led by Canadians), Canadian militia, and sixteen hundred American Indians, all commanded by the Marquis de Montcalm. Promised relief never materialized, and the mixed English and Provincial American garrison capitulated under honorable terms. As paroled prisoners and their families left the fort, Montcalm's bloodthirsty Indian allies fell on them, massacring men, women, and children while the French did nothing. This traditional interpretation endures in histories and legend as typical of British inefficiency, French perfidy, and, especially, American Indian barbarity.

Ian K. Steele artfully blends cultural and military history to force readers to rethink this longheld historical conventional wisdom. Warfare in America incorporated many European practices, but formal European niceties such as prisoner exchanges and paroles meant little along the frontier, particularly to American Indian warriors. To them, returning home from battle with prisoners, scalps, and loot were symbols of manhood, greatly enhancing individual prestige. Moreover, selling English captives to the French (who then ransomed them) had developed into a lucrative component of the Indian economy; live prisoners were valuable commodities. To Indian perception, therefore, French promises of booty that lured them into accompanying Montcalm naturally included prisoners of war. When the parolees departed with their personal belongings as permitted under European convention, the Indians felt betrayed. They angrily fell on the hapless prisoners (who, too, felt deceived by French assurances of safe passage), seeking their rightful spoils of war. Quite a few prisoners died, but many more were taken to Canada, sold, and later repatriated. Causality actually

stemmed from a much more complex set of imperatives: a clash of European and Indian cultural perceptions. As Steele points out in an excellent historiographic chapter, most historians failed to recognize (or ignored) American Indian perceptions and instead kept alive the murdering savage stereotype.

Extremely well documented from research on both sides of the Atlantic, Steele treats Europeans and American Indians fairly and is unafraid of presenting either side's warts and blemishes. In discussing cultural clashes that precipitate the "massacre," however, he mentions but fails to expand on relations among other participating cultures such as the *troupes de la marine*, French regulars, Canadian militiamen, and the *coursier des bois*. Although he also questions tired eighteenth-century clichés such as Indians smashing babies' heads into trees, he ignores the work of Howard Mumford Jones, David Beers Quinn, and James Muldoon who show such racially motivated imagery as originating in sixteenth-century English anti-"Wild Irish" literature. Nevertheless, this study, as with any good history, is multidimensional and thought provoking in its most positive sense. It is a superbly crafted, well-researched study of mid-eighteenth-century North American military culture.

JOHN MORGAN DEDERER
Fairfield, Connecticut

WILLARD STERNE RANDALL. *Benedict Arnold: Patriot and Traitor.* New York: William Morrow. 1990. Pp. 667. \$27.95.

During the dark days of 1777, Benjamin Rush complained to John Adams that George Washington lacked the ability to win independence for the United States. Rush was wrong to think in such a manner, Adams responded. The success of the American rebellion, he added, did not hinge on the actions of any one man.

Adams was very nearly wrong. By 1780 the actions of one man, Benedict Arnold, might have ruined America's bid for independence. By 1780, chronic fiscal woes, conservative-radical divisions over the course of the domestic revolution, catastrophic recent defeats at Camden and Charleston, and growing pressures in Europe for a mediated solution of the protracted war had combined to imperil the new nation's chances of separating from Great Britain. In the spring of that year, even Washington said that he had almost lost hope of achieving victory and independence.

It was in these perilous times that Arnold sought to deliver both the American installation at West Point and General Washington to the British. Arnold's treachery might easily have triggered a series of events that would have been fatal to American independence. Had Arnold succeeded in putting West Point in British hands, the French army in Rhode Island, isolated from the Continental army, might

easily have been overwhelmed by Sir Henry Clinton. Had New England been cut off from the other states as a result of Britain's naval prowess and its control of the Hudson, the region might have been squeezed into submission. Or the egregious harm done by Arnold might have induced France to sever its ties with its American ally.

Alas, Arnold's plan failed. Nevertheless, for many his treason continues to be the single most dramatic event of the war, and Arnold, next to Washington, remains the most fascinating military commander who served in this conflict.

Arnold's story has been told often but never in a more readable manner than that provided in this study by Willard Sterne Randall. A journalist who turned to academe, Randall is a gifted writer. His poignant account of the travail of Arnold's men in the march on Quebec in 1775 is unmatched; his ability to simplify complex military engagements is a sheer joy. For any reader who wishes to understand the forces that produced Arnold's treasonous conduct, this fast-paced account is the best now available.

Therein lies something of a problem, however. Instead of a genuine biography, Randall has constructed a book that seeks to explain Arnold's treason. John André, the tragic British officer who paid dearly for his role in Arnold's treason, is introduced into the narrative as early as September 1775, and Arnold's conduct during 1778–79 is assayed largely in terms of how it might help explain his subsequent behavior. Moreover, once past the frenetic events of September 1780, Randall appears to lose interest in his subject. Arnold's year of military activity on the British side is disposed of in nine pages, and the final twenty years of his life merit only twenty pages, less by one-third than the space allotted to the events that transpired between September 10 and September 24, 1780.

Randall breaks little new ground with regard to the causes of Arnold's treason, but his explanation of the general's conduct is sound and judicious and might even elicit sympathy for the traitor among some readers. Only his suggestion that Arnold's loss of Washington's favor nudged him toward his treachery seems to me to be ill-advised.

Despite a few inconsequential errors—Washington was not classically educated, for instance—Randall has produced an engaging study that provides not only an excellent interpretation of the character of Arnold but also a balanced and insightful assessment of the factors that resulted in his defection to the British in 1780.

JOHN FERLING
West Georgia College

ROBERT ALLEN RUTLAND. *The Presidency of James Madison*. (American Presidency Series.) Lawrence: University Press of Kansas. 1990. Pp. xiii, 233. \$25.00.

Most readers will begin this book with the impression that James Madison was a great man whose reputation justly suffered when he became president. Those views will generally remain unchanged unless readers are as over-awed by the last six pages of this book as Robert Allen Rutland obviously was by the last two years of Madison's political career.

Rutland honors Madison for finally winning respect for the United States by not quite losing a war with many comic opera overtones. It required miracles at New Orleans and Ghent and the suicide of the Federalist party, but Madison retired with the Union secure, which was his ultimate priority. John Adams conceded that "notwithstanding a thousand faults and blunders," Madison's administration "established more Union, than all three Predecessors, Washington, Adams and Jefferson put together" (p. 209). Rutland justly compares Madison's last triumphant years in office favorably with Thomas Jefferson's declining years as president.

Yet Rutland is more often dismayed than delighted with Madison's stewardship in the first six years: the phrase becomes an anomaly when Madison's Whiggish conception of the presidency is compared with Theodore Roosevelt's "Stewardship Theory." Madison sought harmony in his appointments and instead got mediocrity. He risked all with his kindly tolerance of unreliable and unfaithful appointees. He hated to admit when he had erred. He was hoodwinked by Napoleon's pretense at fairness to neutral American shipping. He angrily rejected British overtures to prevent war. He acted heroically but ineffectually as the British casually assaulted the nation's capital city.

Rutland successfully distances Madison from presidents Ulysses S. Grant, Warren G. Harding, and Herbert Hoover. But how many seriously put Madison down with the presidential dregs, anyway? Rutland angrily refutes Samuel Eliot Morison's reference to "stupidity" in describing Madison, but Morison's comment was at least foolish, if not stupid. Whatever his inadequacies, Madison was a safer resident of the White House than any contemporaries who yearned for the presidency. After Madison—perhaps even before him—Albert Gallatin is Rutland's hero, but Gallatin was too much a realist to envision his European manners in the White House.

This is the twenty-second volume in the American Presidency Series. Some of the giants are still awaiting their niche: Andrew Jackson, Abraham Lincoln, Woodrow Wilson, and Franklin D. Roosevelt. Rutland is uniquely qualified to describe Madison's presidency. He has lived with Madison much of his adult life, from his doctoral dissertation on the Bill of Rights to the editing of most volumes, to date, of the definitive *The Papers of James Madison* and two volumes of its presidential series. Rutland's fourth recent study of Madison combines thoroughness with wit to an appealing degree.

This volume is least impressive when dealing with the judiciary. It declares that *Marbury v. Madison*

voided the Judiciary Act of 1789 (much of which is still in effect) rather than one very minor section of the act. It speaks of "the layman's" views on *Fletcher v. Peck* and then proceeds to join with those laymen and Jefferson in denouncing the decision.

DONALD O. DEWEY
California State University,
Los Angeles

WILLIAM W. FREEHLING. *The Road to Disunion*. Volume 1, *Secessionists at Bay, 1776–1854*. New York: Oxford University Press. 1990. Pp. xii, 640. \$30.00.

William W. Freehling provocatively sorts out and details the antebellum South's tangled political world and the way that sectional extremists, working originally with unpromising material, ultimately succeeded. It is not an unfamiliar story, but Freehling's rich exploration textures it as never before while ably demonstrating how complex the section's behavior was to 1854. Beginning with an extended essay on southern society, he delineates both extremist determination to brook no challenge to slavery and why other southerners, even slaveholders, were never as the extremists wanted. There was too much economic, social, and intellectual variety in the South and, in consequence, a divided political outlook, largely along regional lines. Although slavery was a vigorous, profitable institution in the Deep South, its role and the way it was viewed greatly changed as one moved further north. The Upper South's conditional terminators saw an eventual end to the institution whenever conditions, particularly the racial complications, were right—which they never quite were. Unconditional perpetualists in the Deep South had no such vision and were always touchily on guard against perceived threats. Complicating matters further, slaveowners absorbed the dominant republicanism of their age differently, fundamentally disagreeing among themselves about class, majority rule, race, and, consequently, politics. The two-party system, no artificial construct, separated them as well and focused attention on issues outside the slavery complex.

Every political battle underscored how divided the section's leaders were and how irresolute or indifferent many were about the need to defend slavery aggressively. Each episode, from Missouri and the gag rule to California and Kansas, tested the waters, developed arguments and dilemmas, even as each provoked awareness, irritated scars, and nudged matters along. Usually, extremists had to back off. Nevertheless, over time, they were able to sharpen battle lines, induce commitment, force demands on their fellow southerners and on their northern Democratic allies, and win once-unthinkable divisive guarantees. The process was never easy and only possible because of the extremists' cohesiveness and determination against those less alert, less well organized, and less sure of what, if anything, was necessary. In short,

slavery counted in southern (and national) politics; it forced attention and compliance, but only gradually, painfully, and in complex fashion, amid an intricate sectional and national political culture.

To summarize Freehling's argument barely begins to do justice to his detailed, nuanced, and highly sophisticated rendering. He has read the existing scholarship thoughtfully, uses it with great care, celebrates much of it, while quietly correcting many shadings and directions. In pithy language and formulations that are sometimes awkward and occasionally allusive, but always quite fully explored, Freehling well presents a powerful argument. What is lost in crispness is made up in depth and insight. Not everyone will agree with his perspectives. It is possible to argue with certain emphases, or cast them into different form. Nevertheless, Freehling's interpretation allows us to bring together much that has divided scholars and to see how the South was both part of the American political nation in the antebellum period and outside of it as well. This is a stunning achievement.

JOEL H. SILBEY
Cornell University

WILLIAM CHEEK and AIMEE LEE CHEEK. *John Mercer Langston and the Fight for Black Freedom, 1829–65*. (Blacks in the New World.) Urbana: University of Illinois Press. 1989. Pp. 478. \$34.95.

John Mercer Langston has long been one of those second-tier political leaders that historians occasionally quote or cite as an example to prove some generalization. His life (1829–97) was so varied and full, his betwixt-and-between individuality so rich in experience, paradox, and accomplishment, that a full-fledged biography has been sorely needed. To date, his autobiography, *From the Virginia Plantation to the National Capitol* (1894), has had to suffice. Now William Cheek and Aimee Lee Cheek have provided us with volume one of a projected two-volume biography that promises to be more than we ever expected on Langston, perhaps more than we need. Their work is not a narrowly focused study of an individual; rather, it places Langston's life fully in the whole panoply of free black life in Ohio. Unlike older life-and-times biography in which the backdrop was boiler-plate history against which to highlight the hero or heroine, background material in the hands of the Cheeks is an impressively researched social and political history of free blacks in Cincinnati and Oberlin. This is really two books in one, a sensitive biography of a black leader and a full-scale history of the society in which he matured and began his career.

Langston's life merits attention and study. Born in Virginia the son of a white planter, Robert Quarles, and a black woman, Lucy Langston, who had been freed by Quarles in 1806, John Mercer Langston—light skinned and free—shared his father's estate

when Quarles died in 1833. The young boy then went with his older brothers to Ohio, where for a period he was reared in a white family. He was taken from that family when they decided to move further west, and thereafter he lived amid the free black community of Cincinnati. Subsequently moving to Oberlin to enroll in college, he there met both subtle and rampant racism but also honed his leadership and speaking skills. He, too, in his experiences in Cincinnati and Oberlin, gained confidence for himself and for his race. Quick witted, with a sharp tongue and a sense of destiny, he emerged as a bold leader of his people. Half white, he fully identified with his oppressed black brothers and sisters and for a while early in his career supported the idea of black emigration from the nation that mocked its own ideals of liberty and justice.

National events constantly intruded on Langston's life and career, but he was no passive observer. In 1854 he became the first black lawyer admitted to the bar west of the Appalachians, and he eagerly, energetically, and effectively used his legal and oratorical skills on behalf of blacks. The times were ripe for such a black leader as Langston had become, and with the fugitive slave controversy, *Uncle Tom's Cabin*, John Brown's raid, then civil war and the opportunity to raise and support black troops, Langston emerged as a powerful force in Ohio and beyond. This volume ends with his wartime endeavors, but, in the more than three decades left to his life, Langston became a general inspector for the Freedmen's Bureau, organized and became dean of Howard University's law school, was American minister and consul to Haiti, president of Virginia Union University, and, near the close of his life, a U.S. congressman representing Virginia. But for all of this we must await volume two.

The Cheeks present every detail of Langston's early career, having combed every conceivable record and obtained every piece of relevant information. All is reported with precision, situated in a historical context that preserves every nuance of meaning. The research that went into this volume is staggering, and Langston's life is revealed affectionately but not sentimentally. The writing style is sometimes tiresome, with too many overlong sentences, and the detail is occasionally smothering. After this two-volume work is completed, every specialist in the field will be thankful. General readers will then need an abridged biography that sifts the specifically Langston material from the mountain of detail. But make no mistake; this project will be mined by scholars for years.

JOHN B. BOLES
Rice University

MICHAEL TADMAN. *Speculators and Slaves: Masters, Traders, and Slaves in the Old South*. Madison: University of Wisconsin Press. 1989. Pp. xviii, 317. \$27.75.

This detailed, meticulously researched and documented analysis of the internal (domestic) slave trade represents historical scholarship at its best. Michael Tadman accessed archives from a dozen states, including court, probate, and sheriffs' records, contemporary census and newspaper accounts, as well as coastal manifests. In addition, he builds well on the work of others, showing a comfortable familiarity with modern contributions to the field of slavery studies. Moreover, the author makes extensive use of slave traders' correspondence and account books, a virtually untapped gold mine of primary source material.

Dealing primarily with the four decades preceding the Civil War, Tadman convincingly argues that the interregional slave traffic between the Upper and Lower South was dominated by slave trading (that is, the result of sales to and by speculators) as opposed to planter migration. This, of course, runs contrary to the popular view advanced by Robert Fogel and Stanley Engerman, among others, that trading was numerically less significant than planter migration in accounting for the overall volume of the traffic.

More significant, Tadman maintains that the long-entrenched myth of planter paternalism as well as the modern "accommodation" theories of master and slave relationships clearly are out of step with reality. Both sellers and buyers, he argues, were practical, profit-driven individuals, propelled by considerations of purely economic self-interest. The massive and deliberate abuse (separation by sale) of slave families, he maintains, was the inevitable result. This abuse, in turn, provided slaves with a crucial gauge by which they measured the real intentions of their masters and judged the slavery system in general. Invariably, both came out wanting.

Tadman additionally challenges the widespread assumption that the slave trade was fueled by a systematic, widespread network of slave breeding "stud farms" in the Upper South. Slave breeding theories, he argues, are not sustainable in light of the available evidence or lack thereof.

Some may take exception to Tadman's attempt to dismiss the illegal continuation of the Atlantic slave trade after 1808 as numerically insignificant and inconsequential. To argue, for example, that newly imported Africans in the fifty years following legal proscription were "very few in number" is wishful thinking at best (p. 11). To argue further that "there are virtually no documented cases of smuggling and no reports of unusual, unacclimatized, foreign-speaking newcomers in the slave population" after 1808 compounds the exaggeration (p. 239). To be sure, recent scholarship clearly has demonstrated that illegal importations were not numerically extensive after 1808, but to write off those that did occur distorts reality.

But this is a minor point, and by no means does it detract from the overall significance of Tadman's achievement. This study is must reading for all of

those interested in African-American history and in the complexities of southern slave society.

ROBERT R. DAVIS, JR.
Ohio Northern University

JOHN R. MULKERN. *The Know-Nothing Party in Massachusetts: The Rise and Fall of a People's Movement*. Boston: Northeastern University Press. 1990. Pp. ix, 236. \$35.00.

The secretly organized American (Know-Nothing) party won every statewide elective office in Massachusetts in 1854, all Senate seats, all but three places in the lower house, and the entire congressional delegation. The Whig and Democratic parties, already weakened by internal discord and desertions, were repudiated by an aroused electorate. John R. Mulkern seeks to explain this extraordinary upheaval.

Rejecting traditional views, Mulkern plays down the influence of national politics on the state scene and discounts the impact of such issues as nativism, antislavery, and temperance. Rather, he insists with some fervor that the Know-Nothings represented "an antiparty, antipolitical populist movement" (p. 5) that "mobilized the urban poor and working class behind it" (p. 179) with the aim of "radically transforming the political and economic system" (p. 184). Mulkern relates this populist explosion to the preeminence of Massachusetts in building a new industrial order that brought about wrenching social and economic dislocations. Because the second-party system failed to address these problems, people lost faith in the old parties and turned to the Know-Nothing lodges as an alternative.

A few years earlier, Free Soilers and Democrats had formed a "Coalition" that succeeded in overthrowing the long-dominant Whigs, enacting several reform measures, and initiating a call for a constitutional convention. The Know-Nothing legislature of 1855 carried this impetus forward. Although it gratified its nativist core by passing several acts hostile to immigrants and acted as well on behalf of its temperance and antislavery constituencies, it also displayed reformist zeal in other areas. It advanced the rights of women, improved the public school system, abolished imprisonment for debt, created new regulatory agencies, and approved constitutional amendments (later ratified) that made the government more democratic. But two causes dear to the hearts of workingmen met with defeat: the ten-hour day and the secret ballot. In Mulkern's estimate, the legislature "chalked up a record for change unsurpassed in state history" (p. 169).

This "genuine people's movement" (p. 68) was immediately captured by designing politicians. Governor Henry J. Gardner, at heart a Brahmin Whig, shrewdly built a personal machine loyal to his own interests. Senator Henry Wilson exploited the movement to aid the antislavery cause. Nathaniel P. Banks

manipulated the fusion of large elements of the Americans with the newly formed Republican party. By 1857 only a small remnant of Know-Nothings remained. As the lodges collapsed, most of the erstwhile members enlisted in the Republican ranks, some returned to their original Democratic allegiance, and many simply dropped out of the active electorate.

This sprightly narrative is more notable for its strongly argued thesis than for its substantive contribution to our knowledge of the Know-Nothings. Mulkern has extended the insights developed by Michael F. Holt and Ronald P. Formisano to depict the Know-Nothings in a guise that tests our credulity. Nevertheless, his synthesis of both the older and more recent scholarship, including the important analyses of William F. Gienapp and Dale Baum, makes this work useful as well as provocative.

RICHARD P. MCCORMICK
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VERNON L. VOLPE. *Forlorn Hope of Freedom: The Liberty Party in the Old Northwest, 1838-1848*. Kent, Ohio: Kent State University Press. 1990. Pp. xxii, 236. \$24.00.

The abolitionists' failure to secure slave emancipation by establishing their own political party has been well studied by Richard H. Sewell, Alan Kraut, and Stephen Maizlish, among others. And, in the work of Lawrence Friedman, the private visions and shared experiences of several of the Liberty party's principal organizers have also been covered. Vernon L. Volpe's extended analysis of the rise and collapse of the western Liberty party from 1838 to 1848 more often builds on the findings of these scholars than departs significantly from them.

In this respect, the central argument of the book is difficult to dispute, for it has long been established, as Volpe contends repeatedly, that adherents of the Liberty party were hardly kin to the enthusiastic partisans who enrolled as Whigs and Democrats. Their votes for legislated emancipation were far more often expressions of deep piety and personal alienation than reflections of partisan zeal or economic conviction. Their challenge to the two-party system instead bespoke abiding suspicions of the "corruption" of politics and fears of the nation's spiritual decline. Readers familiar with the works of historians as diverse in perspective as Lee Benson, Ronald Formisano, Ronald Walters, Bertram Wyatt-Brown, Lewis Perry, and John McKivigan will find little new in such conclusions or in Volpe's central proposition, which is stated on the back flyleaf, that "to evaluate the Liberty Party's role properly, emphasis must be directed from political to religious channels."

But, if Volpe often elaborates on the familiar, he does so cogently and generally to good purpose. In

fact, he explores more deeply and revealingly than any previous author just who the rank-and-file supporters of the western Liberty party were and why they chose to take up so impotent an approach to slave emancipation. In the Old Northwest, Volpe finds, Liberty party voters drew from their activism the spirit of a surrogate religion. To them, involvement in the Liberty party represented a soul-cleansing alternative to further communion with orthodox religious denominations that had rejected their pleas for the slave. By joining the Liberty party, these spiritually frustrated Christians sought not only black liberation, Volpe argues, but also their personal emancipation from an overwhelming sense of personal and national sin. To support these contentions, Volpe carefully examines an impressive range of voting returns, memoirs, contemporary accounts, newspapers, county histories, and manuscript collections. From such evidence, he builds a convincing case that Liberty party voters could be found most frequently in isolated, homogeneous communities where pious citizens felt increasingly threatened by the intrusions of the national market, the mass party system, and the "slavepower." Far from acting as exemplars of middle-class capitalism, most of these rank-and-file abolitionists cast their ballots as defenses against encroaching modernity.

In addition to furnishing this valuable cautionary against equating abolitionism too rigidly with the free market (without, however, trying to revive David Donald's "status politics" thesis), Volpe also makes other important contributions. His treatment of the presidential election of 1840, for example, demonstrates that the second-party system was even more bellicose about protecting slavery and white supremacy than previous scholarship has indicated. In the Midwest, at least, "hard cider" mixed easily with harsh premeditated strategies of racism and anti-abolitionism on the part of Whigs no less than Democrats, a disturbing harbinger of the still greater abuse that the Liberty party would endure in the elections of 1844. In addition, Volpe draws a number of careful and extended distinctions between not only the Liberty party's western and eastern branches but also between the strategies of its leaders versus the preferences of its rank and file. As a result, Volpe is convincing when emphasizing that scholars have relied overmuch on the opinions of certain party leaders and too little on the views of the electorate when accounting for the party's internal tensions and doctrinal evolution. Although not a venturesome departure from established scholarship, Volpe's careful study of the Liberty party clearly enriches our understanding of the political war against slavery.

JAMES BREWER STEWART
Macalester College

GLENN C. ALTSCHULER. *Better than Second Best: Love and Work in the Life of Helen Magill*. (Women in American

History.) Urbana: University of Illinois Press. 1990. Pp. 175. \$29.95.

The lives of many individuals left out of political records we now know provide insights into neglected dimensions of the past. Today's women's studies programs are rich in autobiographies based on letters and diaries and on formal biographies that put new women's voices on our library shelves. Helen Magill's is such a life. Reading Glenn C. Altschuler's biography of the first American woman Ph.D., documented from letters and Magill's diary, we should all appreciate how difficult it has been for intellectual women to find a place in a culture that caters to conformity. Like Margaret Fuller, Magill remained a brilliant outsider in spite of being the daughter of the president of Swarthmore and the second wife of the president of Cornell.

In writing about Magill, Altschuler not only helps us appreciate the achievements of a driven life but also makes us wonder about the society that consistently managed to put a woman who was "better than second best" in second place. Perhaps unconsciously the emphasis in writing such lives is shifting away from "valorizing" stalwart women who carry off small trophies to suggesting that institutions could change in the service of the gifted. We still waste minds.

In some sense, the reader is tempted to see the subtitle as ironic. Sigmund Freud's requirements for a good life are only offered in part to the earnest Magill. And she becomes more and more crotchety. Paying frequent tribute to the many thoughtful historians of women now trying to explain women's lives, Altschuler thinks Magill would have done better with a circle of women friends. But she did have one serious correspondent, Eva Channing; and Magill was also one of five sisters. Her commitment to expanding women's possibilities rather than accepting what was offered was asking for trouble. Like Ann Hopkins, who recently won a partnership suit against Price Waterhouse, Magill was an aggressive individualist; she would have been a sure candidate for the "Charm School" Hopkins was said to need.

Born in 1853 of Hicksite Quakers, Magill was educated to speak her mind, but she was also trained to serve. Like most women she felt the terrible bind that put love and work at odds in ways rarely experienced by men. Had she been able to see marriage as the role for failures, as M. Carey Thomas's Bryn Mawr students later could, she might have been happier. But marriage was just as much her goal as demonstrating that women's intellects were equal to men's.

Magill's first triumph was to be the only girl at Boston Latin school where her father taught. Although she refuted Edward Clarke's assertion that the female constitution would be weakened from studying a male curriculum, Magill was not invited to attend Harvard with the other members of her class. Instead, she became a pioneer in coeducation at

Swarthmore, enhancing her father's dreams. Afterward admitted to Boston University for a Ph.D. in classics, she dubbed the university "Blessed Utopia." Finally, after receiving her Ph.D., she went on also to become the first American woman to pass the Tripos at Cambridge. But none of these achievements won her a decent job. Marrying a man as distinguished as Andrew White, she thought, would provide her with a stimulating companion. But White kept a picture of his first wife in every room and made it all too clear that the second was no equal to the Angel who looked after him for so many years.

Altschuler has given us much to ponder about the tensions educated women have experienced and still experience. The Magills are a remarkable family; perhaps when scholars delve into the diaries of the other four sisters, we shall have an unprecedented picture of nineteenth-century women.

EUGENIA KALEDIN

Alliance of Independent Scholars

MARY DENIS MAHER. *To Bind Up the Wounds: Catholic Sister Nurses in the U.S. Civil War*. (Contributions in Women's Studies, number 107.) New York: Greenwood. 1989. Pp. x, 178. \$39.95.

This book explores the courageous and tireless efforts of Catholic sisters who nursed Union and Confederate soldiers during the Civil War. Mary Denis Maher displays deep compassion for her subject, defending the nuns' selfless contributions, superior nursing skills, and organizational abilities. As the only source of trained nurses during the war, according to Maher, Catholic sisters became "pathbreakers" in opening the nursing profession to women.

The author places the nuns' service in a historical context, providing background into Catholicism in America, the sisters' unique preparation as nurses, and nineteenth-century medical practices. Trained to endure hardship and live a simple life, nuns apparently rarely complained, whatever dangers or hardships they confronted. During the war, nuns served in varied settings and performed a wide range of duties, including cleaning, cooking, nurturing, dispensing supplies, and preparing corpses for burial. Nevertheless, not all six hundred sisters who served exhibited patience and obedience, for some successfully challenged hospital administrators and doctors, demanding better food, clean linen, and ventilated rooms for the soldiers.

Not only did sisters improve the quality of health care but they also successfully countered criticism directed at them as female nurses and Catholics. During a period of marked xenophobia, nuns withstood public criticism of their clothing and spiritual influence on their patients. They adopted the neutral stance of the Catholic church, causing many to question their loyalty during the war. But, as nuns, they possessed characteristics highly valued by medical

authorities, including obedience, flexibility, dedication, and chastity. Eventually they earned the grudging respect of physicians and frequently the affection of their patients.

Any book that enlarges our understanding of women during wartime and especially their role as nurses should be viewed as a positive contribution. Unfortunately, Maher's study contains problems that detract from the importance of the topic. The publisher should have paid closer attention to editing, for the book is repetitive and contains spelling and grammatical errors. In demonstrating the nuns' endless contributions, Maher adopts a defensive position. She diminishes the role of lay nurses, frequently reminding the reader that these women were often unprepared and ill-suited to serve. By claiming that only Catholic sisters were properly trained as nurses, she ignores the skills that many women possessed as medical caretakers of their families. Hundreds of civilian women in both the North and the South served selflessly and became increasingly proficient as the war dragged on. All women who contributed to this ghastly war worked valiantly and tried to lessen the suffering and shockingly high mortality.

SALLY MCMILLEN

Davidson College

ERNEST A. MCKAY. *The Civil War and New York City*. Syracuse, N.Y.: Syracuse University Press. 1990. Pp. xv, 377. \$34.95.

To read this book is to step back into the past, not only to the Civil War era but to earlier days of historical writing. Ernest A. McKay writes that his purpose is "to tell about the thoughts and actions of important and unimportant people" (p. xi), and this he does with considerable verve and style, without resort to statistical analysis, social science theory, psychohistory, or any other historiographical fashion of the last thirty years.

Using New York City as his stage, McKay unfolds a narrative in sixteen acts that begins with the election of 1860 and ends with Union victory in 1865. There are numerous characters in this drama. Some appear throughout, including one of McKay's favorites, the brilliant rascal Fernando Wood, and a distinct nonfavorite, the journalist Horace Greeley. Others have shorter roles, such as General Benjamin "Beast" Butler, who in 1864 commanded troops sent to the city to maintain order in the presidential election, while Charles Godfrey Gunther, a pacifist and critic of the war, presided as mayor. Although most of the characters are males, McKay provides room for women such as Elizabeth Blackwell, the first American woman to get a medical degree and the organizer of a training course for military nurses.

McKay is particularly alert to the inconsistencies of human behavior. He gives special attention to the Irish, a people often bitterly hostile to the war but

heavy contributors to casualties for the Union cause. And he notes how New York, a city conspicuously indifferent to the evils of slavery and the plight of black people, eventually accepted emancipation and applauded the enlistment of black men in the military.

Generally, the war is a distant roar, but on occasion it comes to the fore: New Yorkers showed both cowardice and courage at the First Battle of Bull Run; preparations were made to defend the city against Confederate naval attacks; a rebel raider actually briefly terrorized New York's shipping; and in July 1863, discontents exploded into the terrible draft riots.

There are some weaknesses here. In his rare attempts to generalize, McKay tends to be preachy, especially in a clumsy concluding homily on compromise in preference to war, and his instinctive conservatism may be bothersome. More basic, he often treats New York merely as a stage for his story, neglecting the city's physical and institutional development. McKay is not indifferent to organizational life, providing interesting accounts of the Union Defense Committee and the U.S. Sanitary Commission, and he does give some attention to political, social, and economic matters. But, lacking the skills and interests of the urban historian, he falls far short of providing a careful study of life and change in the great American metropolis during the Civil War period.

McKay's work, however, deserves to be read. Although it says little that is *fundamentally new*, it provides a broad panorama of war-torn times that sparkle with new details and insights. Above all, it demonstrates how the artful narrator can not only tell an interesting story but also give human meaning to the past.

EDWARD K. SPANN
Indiana State University

STEVEN E. WOODWORTH. *Jefferson Davis and His Generals: The Failure of Confederate Command in the West*. (Modern War Studies.) Lawrence: University Press of Kansas. 1990. Pp. xv, 380. \$25.00.

Steven E. Woodworth's valuable study of Jefferson Davis and his generals in the western theater is intended to reveal a Davis "much different from the man described by either his defenders or his detractors" (p. xiv). Davis does not deserve, Woodworth argues, some of the harsh criticism expounded by his detractors, nor was he the self-assured military genius portrayed by his defenders. In arguing this thesis, Woodworth is partially successful.

Davis did have his moments as commander in chief of Confederate armies, especially, as Woodworth demonstrates, from the beginning of the war to Shiloh. He was adequate through 1862, but, from 1863 to 1865, Confederate fortunes predictably par-

alleled those of its leader. The author appreciates Davis's "brave but unavailing perseverance" during the latter stages of the war but is quick to point out damage to the Confederate cause as a result of Davis's refusal to remove incompetent and disobedient generals (p. 306).

Woodworth is less convincing in his assertion that Davis was not a confident leader who believed in his own military expertise. The view that, "deep down," Davis "might entertain a degree of uncertainty about his abilities as a grand strategist" is not proven (p. 89). True, Davis did have moments of indecision, especially after the tide turned against the South. But his indecisiveness was perhaps more a result of personality flaws (blind loyalty to friends, for example) than of a loss of confidence in his military acumen.

Beyond the thesis on Davis, this volume will surely raise eyebrows because of Woodworth's attempted resurrection of Braxton Bragg's reputation. Woodworth makes a good case that Bragg was better than he has been portrayed by contemporaries and historians. In many cases Bragg was a victim of conspiracies by his subordinates, but his own shortcomings were considerable. Woodworth makes a valiant effort, but he will probably convince few readers. His treatment of other generals is equally tantalizing though generally more predictable.

Unfortunately, the author has perpetuated a couple of old historical myths. Edwin Bearss has proven that Ulysses S. Grant did not abandon his supply line when he crossed the Mississippi in May 1863. Grant maintained a line of wagon trains that kept his army supplied until the beginning of the siege. Woodworth errs in denouncing Davis and John Pemberton for their strategy in going after a nonexistent supply line. The line was there; Pemberton merely waited too long to act. Historical evidence is also strong that James Longstreet did not single-handedly lose the battle at Gettysburg. William Piston and others have shown that Longstreet was a victim of postwar venom by supporters of Robert E. Lee. There were many Confederate culprits at Gettysburg. Longstreet probably the least of them.

Despite some flaws, Woodworth has produced an excellent companion volume to Thomas Connelly's study of Confederate command in his two-volume history of the Army of Tennessee. Woodworth's research is thorough, and his strident, opinionated writing style is provocative and entertaining. His conclusions should provoke discussions and reassessments among Civil War historians. Any work that evokes such results is a worthwhile addition to the limited historiography of the war's decisive theater.

MICHAEL B. BALLARD
Mississippi State University

ERVING E. BEAUREGARD. *Bingham of the Hills: Politician and Diplomat Extraordinary*. (American University

Studies, series 9: History, number 68.) New York: Peter Lang. 1989. Pp. xxi, 327.

John A. Bingham was born in backwoods Pennsylvania in 1815, moved to Cadiz, Ohio, at a relatively early age, studied law, and turned his ambitions to the arena of corporate litigation. He became a Republican congressman for sixteen years, a judge advocate in the prosecution of Lincoln's assassins, and member of the Joint Committee of Fifteen that drafted the Fourteenth Amendment. Defeated for reelection to Congress in 1872, he was appointed by Ulysses S. Grant as Minister Plenipotentiary to the Imperial Court of Japan. Erving E. Beauregard's slender but exhaustively researched biography of Bingham is intended, as the author informs us, to bring a new appreciation to the life of a "highly significant person," a "multifaceted, indeed, Renaissance man," who has "suffered from marked neglect" (p. vii).

Although Beauregard bases his conclusions on a wealth of primary sources and secondary materials, including a large collection of Bingham's letters in private possession, the result in the end is disappointing. The major problem lies in Beauregard's uncritical and nearly devotional enthusiasm for Bingham the man, his ideals, his contributions as a dominant leader of Ohio Republicanism, his influence on the political and economic issues that touched the lives of his constituents, and, to a lesser extent, his love for his wife and family. One cannot fault Beauregard for his admittedly "sympathetic" attitude toward Bingham's personal life and political career, but the task of the biographer is to take us behind the bloated public speeches and highly flattering personal anecdotes from friends and political allies to give us the whole man, warts and all, which would make a biography all the more credible.

It will not do, for example, to tell us that Bingham was involved in the *Crédit Mobilier* scandal of the 1870s and then to marshal evidence in an effort to show that he had not been "corruptly influenced by it," a conclusion that goes far beyond the issue of whether Bingham was nonetheless guilty of unethical conduct. To friends and colleagues, Bingham insisted that he "stood innocent of any wrongdoing" (p. 144). As is so often the case in such political scandals, an indignant and persistent cry of "calumny and abuse" is generally a red flag that at the very least something far more serious than an obviously embarrassing misunderstanding has occurred by the conduct in question.

Again and again, Beauregard points out that Bingham never accumulated a great deal of money and, that after his death in 1900, the family was left with few financial resources. I have no reason to doubt this. At the very end of the book, however, we learn that two of Bingham's daughters were big spenders whose status concerns "dribbled away" his savings and jeopardized the family's financial well-being, not to mention his peace of mind. Bingham, it seems to

me, was an opportunistic type, one for whom what is euphemistically called salary enhancement was inordinately important. All of this suggests that it was family problems rather than "chronic inattention to personal finances" (p. 189) that lay at the heart of Bingham's morally questionable business dealings as a congressman.

Beauregard's treatment of Bingham's role in the drafting of the Fourteenth Amendment does not elevate him into a figure of greater importance than has hitherto been generally recognized. From the work of Jacobus ten Broek, Joseph James, Eric McKittrick, and others, we already know much about the tug and pull of opposing views among members of the Joint Committee of Fifteen before they coalesced to produce the Fourteenth Amendment. Still, it is regrettable that Beauregard did not devote more space in showing us how Bingham may have influenced events or in casting further light on congressional intervention in the Reconstruction process. Instead, after making the dubious claim that the "ghost of the 'Conspiracy Theory' still stalks the earth" (p. 109), Beauregard seems eager to set up straw men—Charles A. Beard, most prominently—and then to knock them down with a barrage of selections from historians whose demolition of the conspiracy theory of the Fourteenth Amendment has for many years been accepted.

On the other hand, Beauregard does provide us with a good deal of information on the factional quarrels within the ranks of the Republican party in Ohio. But here again, such discussions are used mainly as a vehicle for demonstrating Bingham's virtue, wisdom, and political courage. Bingham was an important figure in the party, and he had many friends in his district. But not every person who frustrated his will, disagreed with his policies, or worked against his renomination was (as Beauregard would have us believe) ungrateful, hypocritical, or wrong-headed. There is more than enough evidence to suggest that Bingham was an opinionated, self-righteous politician of considerable ego and a difficult man to get along with. If, as Beauregard argues, Bingham was much sinned against, I suspect it was not always without legitimate cause.

Nevertheless, even those who are critical of Beauregard's willingness to accept Bingham's explanation of events at face value and regret the lack of fresh interpretive insights into the politics of a key northern state during the Civil War and Reconstruction years will benefit from the publication of this biography.

FELICE A. BONADIO
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Santa Barbara*

JAMES LOWELL UNDERWOOD. *The Constitution of South Carolina. Volume 2, Journey toward Local Self-Govern-*

ment. Columbia: University of South Carolina Press. 1989. Pp. xiv, 300.

James Lowell Underwood's second volume of his constitutional history of South Carolina attempts to show how the power of the legislative branch, which was covered in his first book, retarded the growth of viable institutions of local self rule in the Palmetto State. When he focuses on the development of courts, school commissions, church parishes, and other local agencies, the pattern is the same: the General Assembly's domination. First of all, such control meant the fragmentation of local government. South Carolina was never permitted by its legislature to evolve much beyond the "highly splintered nature of local government" of the eighteenth century (p. 239). Moreover, the fragmentation was exacerbated on the one hand by indirection from Columbia and on the other hand by overzealous scrutiny by state officials. The result has been a "convoluted" system of local rule marred by confusion of goals, cumbersomeness, irrelevancies, and, of course, the submersion of local needs to state interests. Even in 1973 an amendment to Article VIII that intended to strengthen local government created more problems than it solved. For example, the old, inefficient, and special-purpose districts persisted, city and county jurisdictions still overlapped, and the proliferation of local boards and commissions continued unabated. The other major problem in the evolution of effective local government was fiscal in nature. The legislature just would not let go of the purse strings. The resulting penurious condition of local authorities meant budgetary juggling and fiscal abuse. The record is discouraging and, Underwood concludes, "the journey toward local self government remains frozen in a state of arrested development" (p. 244).

The author has crafted a well-organized, clearly written exposition of the historical stages of South Carolina's constitution and its relationship to local government: the antebellum years, the Reconstruction constitutions of 1865 and 1868, the document of 1895 that disfranchised black voters, and twentieth-century revisions of 1948, 1973, and 1975. Each stage of constitutional change is related cogently to its predecessors, and Underwood successfully traces the historical motifs in the ties between units at the state and local levels. His scholarship is solid. A total of 775 well-annotated footnotes provide ample ground for any scholar interested in mining further a special interest or topic. The appendix on legislative delegation rule is an informative supplement as is the table of cases cited in this volume. Only a couple of shortcomings come to mind. There is almost nothing (one page) on the fascinating topic of slavery and slave patrols, and South Carolina's experience with the Ku Klux Klan (one page) during Reconstruction is also skipped over. And there is little integration of the debates and issues of the constitutional conventions into the text of the monograph, thus depriving

this study of state constitutional law of much of the color and drama that might have enlivened its telling.

Overall, the volume has much to recommend it to legal scholars and historians. It strongly reinforces the adage that those who do not know the past are condemned to repeat it. With this book in the hands of state officials in South Carolina one hopes some enlightened steps might be undertaken to revamp and to modernize the structure of their local government.

ROBERT P. SUTTON
Western Illinois University

HAROLD E. DAVIS. *Henry Grady's New South: Atlanta, a Brave and Beautiful City*. Tuscaloosa: University of Alabama Press. 1990. Pp. xi, 254. \$34.95.

No one embodied the spirit of the New South more than Henry Grady of Atlanta. His eloquent prose has left contemporary admirers and historians ever since with a highly quotable voice for a region and a movement. The New South was, more accurately, a loosely joined agenda for economic development, agricultural diversification, racial reform, and sectional reconciliation advanced primarily by urban business interests after Reconstruction. Among his qualifications as regional spokesman in this era, Grady added the advantage of dying young, at age thirty-nine, just before the 1890s spoiled so much of the New South dream with a severe depression, a farmers' revolt, and a harsh regime of racial violence and segregation.

Since its publication in 1943, Raymond B. Nixon's laudatory biography, *Henry W. Grady: Spokesman of the New South*, remained the standard work on the subject. A growing body of scholarship on the New South, beginning with C. Vann Woodward's *Origins of the New South* (1951), has revised our understanding of the region in this era and invited a new look at its most famous spokesman. Harold E. Davis is conversant with much of the recent scholarship by economic and social historians of the New South and is attuned to their more critical point of view about its failings. But he does little to recast our image of Grady within this revisionist framework. His main argument is that Grady was more interested in advancing the narrow interests of Atlanta than in promoting the whole region. This seems convincing enough but does not require us to rethink Grady's ideas or the meaning of the New South creed. In the nineteenth century, any movement for economic development and social reform was always a chorus of local interests appealing to provincial patriotism, with regional and national progress to follow. What was good for Atlanta, Grady would have been the first to argue, was good for the South.

Rather than attempt a complete biography of Grady, the author focuses on the peak of his career in the 1880s and limits his discussion to Grady's activities

and thinking on local politics, agriculture, race, and industrial development. The chapter on race is the most gripping. Even as the ugly new face of race relations appeared all around him in the form of lynching, political disfranchisement, and other forms of degradation for blacks, Grady tried to persuade northerners that only the white representatives of property and intelligence in the South could answer the "Negro question." The voice of black protest did not deter Grady from insisting that most blacks were contented with their status in the South. Borrowing from Joel Williams's typology, Davis casts Grady as a "conservative" within a range of racial philosophies. Grady believed firmly in the natural superiority of whites but accepted blacks as a permanent, essential component of the New South, a race that required protection and uplift, not savage repression as the more radical racists were advocating toward the end of Grady's career. The weakness of Grady's voice in protesting this trend was also one of the chief failures of the movement he spoke for.

DON H. DOYLE
Vanderbilt University

TED OWNBY. *Subduing Satan: Religion, Recreation, and Manhood in the Rural South, 1865-1920*. (The Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1990. Pp. xii, 286. \$29.95.

To Wilbur Cash, the white southern male's "mind" consisted of "two streams"—the puritan and the hedonist—that "could and would flow side by side" with minimal conflict and no hypocrisy (*Mind of the South* [1960], p. 59). William Alexander Percy, the planter-poet, saw the same paradox: good ol' boys could "attend revivals and fight and fornicate in the bushes afterwards" (*Lanterns on the Levee*, p. 149). Wrong, argues Ted Ownby, in this delightful portrait of white culture from the Civil War through the early stages of what is called modernization.

According to Ownby, the two streams flowed contrarily and tumultuously. Evangelical culture (Cash's "Puritan") was feminine and thrived in the home, the church, and at revivals. Hedonistic male culture roared outside, in field and stream, in such public places as the blind tigers and brothels of country towns, at impromptu contests and entertainments, and at circuses. Sinning males occasionally repented in church, then backslid. More often the twin between feminine and masculine cultures never met.

Men's very nature was sinful, passionate, sanguinary. Boys disrupted church services while their elders hunted drunk on Sundays. Hunters took such "joy in binge killing" (p. 36) that animal populations approached extinction. Fishermen dynamited waters. Dogfighting and cockfighting were a pleasure, the bloodier the better; and at circuses males reveled in

dangerous performances and in the fearful symmetry of ferocious cats.

Ownby's subject is not race, but he offers a sensible suggestion that white racism fueled evangelicals' war on male culture: blacks were incapable of self-control, less "human," closer to circus animals and other beasts. So to the evangelicals, rescuing white men from passion equalled their redemption from African-Americans' morbid destiny. A significant dimension of modernization in the South, then, was evangelicals' gradual shift from private exhortation to use of governmental authority in the cultural war. The phenomenon parallels the rise of racial segregation by law.

In the last section of the book, Ownby examines evangelicals' reform triumphs—in separating drinking from church and school, then in outlawing saloons altogether; in banning public profanity, fighting, and Sunday entertainments; and in restricting hunting. Their freedom virtually gone, men inclined to persist in sin were forced into the furtiveness that Cash observed during his adulthood. By the 1920s, masculine competitiveness, too, was controlled—in wholesome contests at fairs and in organized sports. It may be emblematic of Ownby's thesis that today, Hank Williams, Jr., the minstrel of good ol' boy culture, promotes Monday Night Football on television. Presumably southern white men watch passively in mixed-sex bars or more likely in the acrylic comfort of home.

Ownby is very vague about class. His subjects "identified themselves as part of the larger white middle class situated between the plantation elite and the black lower class" (p. 17). And students of religion will find that, for Ownby's purposes, all Baptists were the same, which was about the same as Methodists and Presbyterians. There are no Pentecostal-Holiness folk. Rich in details of blood and booze, this book about sin has nearly nothing of sex. Yet Ownby has surveyed a remarkable range of manuscript and printed sources, and his conceptions are informed by theory not only from women's studies but from anthropology and cultural geography as well.

JACK TEMPLE KIRBY
Miami University
Oxford, Ohio

EDMUND L. DRAGO. *Initiative, Paternalism, and Race Relations: Charleston's Avery Normal Institute*. Athens: University of Georgia Press. 1990. Pp. xi, 402. \$45.00.

Historically, race relations in Charleston, South Carolina, have been distinctive, though not unique. Like the plantation societies of the Caribbean and the cities of New Orleans, Baltimore, and Washington, D.C., antebellum Charleston had a significant number of free blacks within its African-American population. Edmund L. Drago traces the origins of Charleston's Avery Normal Institute to the educational activities

and aspirations of the city's antebellum black elite. Although many free blacks had blood ties to South Carolina's planter aristocracy, Drago argues that, contrary to earlier interpretations, they were mostly artisans and skilled workers and possessed cultural beliefs and a world view that emphasized the middle-class values of hard work, discipline, and service associated with the Protestant ethic. The free blacks' strong attachment to education in the trades as well as in the classics was the result of their experiences as a marginalized group concerned primarily with social advancement and self-improvement.

Avery Normal Institute grew directly out of the activities of the American Missionary Association (AMA) in the city at the end of the Civil War. Through the efforts of Charleston-born black minister-educator Francis Louis Cardozo and the financial support from the estate of Pennsylvania philanthropist Charles Avery and the local African-American community, the teacher-training institution was opened in the fall of 1867 with 438 students and an integrated staff. Over the next fifty years, however, northern white educators served as principals and black teachers were employed only in the elementary classes. Nevertheless, throughout its history, Avery enjoyed the support of the city's black elite, which appreciated its emphasis on classical education while it instilled in the student body a healthy respect for manual training and social service. Ironically, although the majority of Avery's graduates became elementary and secondary school teachers, it was not until the early 1920s and a long campaign led by members of the local branch of the NAACP that African-American educators were allowed to teach in Charleston's separate black public schools.

The heightened level of color consciousness in Charleston, which Drago unfortunately refers to as "intrasegregation," was also not unique to the city. Through extensive oral interviews, Drago collected stories of personal slights and discrimination leveled against dark-complexioned students and faculty at Avery by the light-skinned descendants of the free black elite. Ultimately, with the leveling effects of the Great Depression and the retirement of the most noticeably "color struck" teachers in the 1940s, character and academic achievement came to replace skin color and family background as the primary determinants of popularity within the student body.

By the 1940s Avery Institute represented a type of educational enterprise no longer in favor among AMA officials. More interested in the cooperative "people's movement," "democratic activism," and "intercultural education," they were increasingly dismayed by the traditionalism found at Avery and welcomed the decision by Charleston's public school officials to take over the school in 1947. Then in 1954, citing its desire to decrease public expenditures, the Charleston school board decided to close the school following the renovation and conversion of the local black industrial school into a comprehensive public

high school for blacks. Drago ends his narrative with a description of the civil rights campaigns in Charleston and the role of Avery alumni in the local and national movement.

Avery Institute was the institutional result of northern missionary activity and black self-determinist values operating in the postbellum South. Using a wide array of primary sources and secondary works, Drago does an excellent job of tying the evolution of Avery to the changes in race relations in Charleston. Unfortunately, his lack of familiarity with the literature on the history of black secondary and higher education (he incorrectly states that "no one has ever taken a black institution and studied it over an extended period" [p. 4]) means that he understates the significance of his case study and undercuts his potential contribution to our understanding of the development of classical secondary schools for blacks outside of Charleston.

V. P. FRANKLIN
Drexel University

DAVID R. GOLDFIELD. *Black, White, and Southern: Race Relations and Southern Culture, 1940 to the Present*. Baton Rouge: Louisiana State University Press. 1990. Pp. xviii, 321. \$24.95.

David R. Goldfield tells the story of the struggle for black equality in the context of the evolution of southern race relations over the past fifty years. The South, he says, successfully balanced social transformation with cultural continuity: the civil rights movement profoundly reshaped race relations while maintaining important elements of the region's unique culture. The South emerged from the civil rights struggle with its cultural heritage intact, even strengthened. The civil rights movement loosened the cultural shackles that had bound the South by showing that "southernness . . . was not identical with white supremacy" (p. 148); "by removing the public obsession with race," the movement "enabled whites to regain contact with other cultural elements such as past, place, and manners" (p. 170). In the process, southerners, black and white, "redefine[d] themselves and their region" (p. 118).

Goldfield draws an arresting portrait of the "etiquette of race" that long sustained segregation in the South (p. 1). He shows how World War II intruded on the insularity and isolation of the prewar South and began modestly to reshape elements of southern race relations. He focuses intensively on what he calls the years of confrontation in the southern civil rights movement, a period bounded on one side by *Brown v. Board of Education* and the Montgomery bus boycott, and on the other by the march on Selma and the passage of the Voting Rights Act of 1965. He turns next to the period of consolidation, 1965 to 1976, a time marked especially by an exercise of black political power, a focus on economic issues, and an accen-

tuation of cultural and economic divisions within the black community. And he concludes with the complex elements of the period of confusion—the years since 1976—among them, the mixed achievements of black political power, the controversies over affirmative action and busing, the resegregation of southern education, and the contradictions between the progress of the black middle class and the intractable problems of the black underclass.

Based on comprehensive reading in published sources, original as well as scholarly, this book stands as an intelligent interpretive synthesis. The absence of footnotes is balanced to some extent by an extremely useful bibliographical essay. Scholars familiar with the territory that Goldfield covers will be engaged by his narrative, even though not particularly challenged by his argument or analysis. The work will more likely find its principal readership among non-specialists.

Goldfield's rhetoric is sometimes overblown, as in his account of the Greensboro sit-in: "Much as the embattled colonists harked back to a constitutional regime they had enjoyed prior to 1763, and much as the Confederates depicted themselves as the true heirs of 1787, these teenagers were preservationists: they sought to extend the law of the land to themselves and their region" (p. 118). Sometimes his sympathies for the South yield highly optimistic interpretations—as, for example, in his suggestion that after World War II, "the South appeared to be making good on its oft repeated promise of solving its own [racial] problems" (p. 52); or his assertion that "the fortress of segregation . . . crumbled easily" in the wake of the enactment of the Civil Rights Act of 1964 (p. 146); or his insistence that by the 1980s, "the advances of the civil rights movement [had] become too ingrained in regional life to be threatened from outside or from within" (p. 272). Occasionally he greatly oversimplifies the process of accomplishing change, as in his discussion of the desegregation of public accommodations in Atlanta in 1961 or in his treatment of the passage of the Civil Rights Act of 1964. On the whole, however, the book is intelligent, balanced, and readable—a reliable resource for the reader seeking to understand the complex role of race in the modern South.

NANCY J. WEISS
Princeton University

ANNE FARRAR HYDE. *An American Vision: Far Western Landscape and National Culture, 1820–1920*. (The American Social Experience Series, number 17.) New York: New York University Press. 1990. Pp. xiv, 346. \$29.95.

In this book, Anne Farrar Hyde demonstrates a shift in American perceptions of the West between 1820 and 1920. Until the 1880s, Americans tried to com-

prehend the western landscape as a European analogue; increasingly thereafter, they doted on the West itself as something quintessentially American. Hyde argues that the transformation signaled the emergence of a more confidently nationalistic culture, the West serving as a stand-in for America—shades of Turner!

Hyde establishes the first set of attitudes by examining the literature of nineteenth-century western exploration, painting and photography, and travelers' accounts. Early visitors, she argues, struggled to find a vocabulary adequate to unfamiliar landscape—thus, the reliance on often-inappropriate European analogies. Subsequently these analogies became entrenched in the promotional literature designed to lure wealthy travelers west on the newly completed railroad lines, where luxury resorts in the Old World style awaited them in Colorado and California. The key was to shelter tourists from any confrontation with the region. But by the 1880s Americans began to hanker for a taste of the West. Even as language shifted to a more scientific prose that could do justice to the uniqueness of American nature—geology, flora, fauna—tourists were demanding a real western experience. Wild and woolly was in. Of course, tourists still wanted their comforts while roughing it. Thus emerged the vogue of "rustic" luxury resorts—massive log columns and all—at such touchstone attractions as Yellowstone, the Grand Canyon, and Glacier National Park.

Much of Hyde's argument has been closely anticipated in the work of William H. Goetzmann, Lee Clark Mitchell, and, especially, Earl Pomeroy, whose *In Search of the Golden West* (1957) got the gist of her thesis into a sentence: "for sixty or seventy years . . . tourists had to be reassured . . . that the West was no longer wild and woolly—until fashions changed and it was time to convince them that it was as wild as it ever had been" (p. 89). Hyde adds considerable detail, and her discussion of the railroads and western hotel architecture is very informative. But her thesis is overstated (literally, through excessive recapitulation) and too schematic, consisting of a before-and-after comparison hinging on a turning point that is assumed but not explained. Hyde's pivotal chapter, "The Far Away Nearby: Discovering the Far Western Landscape, 1885–1915," is descriptive rather than analytical. Suddenly Americans are said to embrace what formerly repulsed or embarrassed them. Because the turning point is left implicit in Hyde's set of contrasting images, it is essential that a balance between them be maintained. Hyde offers contrasting chapters on explorers' reports, travelers' accounts, promotional literature, and resort architecture. But her discussion of early visual imagery has no later counterpart, suggesting the lack of a clear division in landscape portraiture and casting doubt on her broader thesis, namely, that the encounter with the

West "forever altered American aesthetic standards" (p. 9).

BRIAN W. DIPP
University of Victoria,
British Columbia

RICHARD HOGAN. *Class and Community in Frontier Colorado*. (Studies in Historical Social Change.) Lawrence: University Press of Kansas. 1990. Pp. xii, 250. \$29.95.

As the title implies, this study deals with community growth and class formation in pioneering Colorado during the first two decades of Colorado's rapid transformation from a remote wilderness to a booming mining and agricultural state. The volume, the sixth in the Studies in Historical Change series, is a worthy example of how historical sociology can provide insights about frontier development that standard histories often fail to do. Richard Hogan's approach is particularly interesting; in order to avoid total reliance on either the consensus or the conflict perspectives in the development of his sociological model, he borrows liberally from both, producing conclusions that seem, on the whole, solid and well balanced.

One of the most important catalysts for Colorado's early growth was the pioneer booster, whose activities were particularly crucial in those frontier communities that Hogan labels carnival towns. These communities, where local governments were very much under public control and used basically to defend personal as opposed to property rights, were dominated by such "laboring classes" as miners, independent artisans, and well-to-do farmers, the latter most numerous in the northern part of the state. Hogan devotes a chapter apiece to frontier Colorado's three most representative carnival communities: Denver, Central City, and Greeley.

Political organization and community growth tended to follow a less democratic direction in the case of the author's second municipal classification, the property-oriented and privately controlled government of the caucus community. Caucus towns were dominated by what Hogan calls "nonlaboring classes," in essence classes comprised of employers whose influence was particularly decisive in transportation and ranching and farming communities where there was a lack of effective competition, such as in those southern Colorado towns where Hispanics were often compelled to work as wage laborers even in agrarian enterprises. Golden, Pueblo, and Cañon City were chosen as most representative of Colorado's fledgling caucus towns, and a chapter is provided for each.

As the economy of the Centennial State matured, especially toward the end of Colorado's second decade, conflict disrupted what had been a rather harmonious and locally controlled process of growth. The increasing presence of monopoly capital from

the East and the continuing expansion of federal authority were the two major threats to the state's early and relatively happy status quo. This conflict took different forms, however. In carnival towns, the struggle was replete with "rancorous labor-capital disputes," whereas in caucus towns, such as railroad and cattle centers where local entrepreneurs and employers were in charge, there were "protracted struggles between local and national capital" (p. 15).

This study is a thorough and conscientious one. There are twenty-seven tables disseminated throughout its pages to quantify many of the author's assertions. But it is also a no-nonsense study. Hogan has covered the most colorful period of Colorado's frontier history without capturing any of its excitement or vitality.

ROBERT W. LARSON
Aurora, Colorado

LEE L. BEAN *et al.* *Fertility Change on the American Frontier: Adaptation and Innovation*. (Studies in Demography, number 4.) Berkeley and Los Angeles: University of California Press. 1990. Pp. xiii, 295. \$40.00.

This study is a massive statistical analysis of some 185,000 family genealogies. Female members of these families, between 1800 and 1899, either migrated to or were born in Utah. The records are drawn from the incredibly large Mormon genealogical archives housed in Salt Lake City, Utah. Information includes date and place of birth (including city and county), marital history, and religious affiliation of parents and children. With more than 1.2 million linked genealogies, this must be the largest effort ever made to trace the fertility of so many over so long a period of time and over such geographic distance.

The authors' stated purpose is to test the accuracy of two key concepts in demographic history: whether the decline in fertility (at least in western nations) was the result of "innovation-diffusion" or of "adaptation." "Innovation-diffusion," in its original formulation by Gösta Carlsson, argued that Western societies were traditionally characterized by uncontrolled fertility, that in the late nineteenth century fertility declined as city folks became aware of effective birth control methods, and that the decline continued as those methods "diffused" to the countryside. The "adaptation" counterargument considers fertility (high, low, or falling) as a rational adaptation to market forces. As formulated by economists such as Gary Becker and Richard Easterlin, children are "consumer durables"; when the cost of obtaining and keeping them (schooling, for example) increases relative to other desirable goods, parents limit their number.

In the process of testing these two theories, the authors provide significant empirical findings. The frontier environment and Mormon affiliation both increased fertility. Women born in 1825 (the pre-

Mormon, premigration era), on average, had seven children during their lifetimes; those born in 1850 had eight. The linking of Utah to the national economy by the transcontinental railroad in 1869 was followed by such a rapid and dramatic decline in fertility that the 1900 birth cohort had "only" five children. Two of the means by which this decline was accomplished are what one would expect: later age of marriage and earlier termination of childbearing. The third is perhaps the study's major contribution: increased spacing of children, a factor developed in earlier articles by Douglas L. Anderton.

Settling the theoretical controversy of "innovation" versus "adaptation" is not clear-cut. The authors reject the "innovation" explanation, but, although one tends to agree with them, doubts remain. One reason is that innovation theory has lately been expanded to include attitudes as well as birth control methods; as a result, the theory is now less precise and more difficult to verify or to refute. Even more troubling for the authors is the fact that Utah's rural areas had lower fertility than urban areas, which suggests some sort of diffusion, perhaps attitudinal if not technical. The authors' explanation of why this is not evidence of diffusion—that urban and rural areas declined together, that fertility differences persisted over time and had different causal factors—is suggestive but not sufficiently developed or proven. Finally, the data base lacks individual educational, economic and occupational information; consequently, the authors must rely on aggregate geographic data to argue that reduced fertility was caused primarily by families' adaptation to forces of modernization and market penetration.

LAURENCE A. GLASCO
University of Pittsburgh

RICHARD A. MECKEL. *Save the Babies: American Public Health Reform and the Prevention of Infant Mortality, 1850–1929*. (The Henry E. Sigerist Series in the History of Medicine.) Baltimore: Johns Hopkins University Press. 1990. Pp. xi, 302. \$42.50.

Infant health is considered the index of a nation's health status. Thus, Richard A. Meckel's study, a historical analysis of the attempts to cope with high rates of infant mortality, proves an important gauge for understanding American society.

From about 1850 to 1880, reformers interested in infant health emphasized the need to clean up the environment. By the last two decades of the century, contemporary studies concluded that the most significant factor in infant health was gastrointestinal diseases. Thereafter, the adverse affects of parental behavior and environmental conditions received less attention than the drive to ensure appropriate food. Physicians sought to create a healthful artificial food, but, by the end of the period, greater attention was

paid to the need for unadulterated pure milk, much of it distributed through milk stations.

Improved milk supplies by the century's end were correlated with a slowly declining infant mortality rate, yet the decrease was smaller than expected and left larger issues unaddressed. From further studies, contemporaries inferred that the ill health of infants resulted from maternal ignorance. Milk stations expanded to include medical consultations and education in child care; visiting nurses brought instruction into the home. Settlement houses and Little Mothers' Leagues provided additional classes. Middle-class women were also instructed through baby-care manuals and Baby Week campaigns. These educational activities served to highlight the role of medicine and push socioeconomic factors still farther into the background.

By the second decade of this century, statistics of those under one month of age were little affected despite declining infant mortality rates. The health of the neonate was intertwined with the mother's health. Moreover, reformers believed, a woman's ability to bear a healthy infant went beyond her taking advice; it was determined by her medical condition. The call for accessible medical care served to elevate obstetrics and eliminate midwives. This medicalization continued the focus on the individual rather than the socioeconomic conditions that influenced her life style and options.

Meckel's clear exposition highlights the shifting scientific and medical understanding, as well as demographics and politics, that shaped interpretations and responses to the problem of infant mortality. The United States was not unique in its concern, and Meckel shows how the actions of other countries influenced developments here. Despite the author's attention to detail, however, the reader is left with several important questions. Who were these "physicians and health reformers"? The medical profession was not monolithic; all health reformers were not medical practitioners. We need to know more about these people. Additionally, we hear almost exclusively from the physicians, reformers, and their opponents on the political scene. What of others intimately involved: the mothers and the public health nurses? How did they view these efforts, especially the medicalization of child care and the privileging of white, middle-class family values?

It is a credit to Meckel that this informative study raises such questions. This book can be read with profit by those interested in the politics of public health as well as the history of medicine.

RIMA D. APPLE
University of Wisconsin,
Madison

ELAINE S. ABELSON. *When Ladies Go A-Thieving: Middle-Class Shoplifters in the Victorian Department Store*. New

York: Oxford University Press. 1989. Pp. ix, 292. \$29.95.

Through a depiction of the conditions producing the female department store shoplifter, Elaine S. Abelson examines the problematic relation of middle-class women, the advance agents in the transition from household production to habits of consumption, to late nineteenth-century consumer culture. This shoplifter, she maintains, symbolized the "unacknowledged implications of consumer capitalism" (p. 9), especially in regard to gender and class.

Abelson describes the emergence of shopping as one of the responsibilities and pleasures of the middle-class American woman, who by patronage of the new, large department stores in the second half of the nineteenth century, helped in the shift of values from "prudent economy to guarded extravagance" (p. 42). In documenting the shopping habits of New York City area women, Abelson contributes to our growing realization that the far from idle middle-class housewife moved about freely in the big city, crossing the permeable boundary between public and private spheres as she shopped, made mortgage payments, and bought insurance policies.

The department stores that the middle-class woman frequented were designed largely with her in mind in the abundance of goods openly and aesthetically displayed and the vast array of services provided to ensure her comfort and ease of purchasing. The stores' "calculated arousal of desire" (p. 11), meant to induce lavish spending, instead instilled in numerous moderately affluent female customers "uncontrollable" impulses to take goods without paying for them. In an especially fascinating chapter in this interesting and well-written book, Abelson describes the use of plate glass windows, glass display cases, and mirrors both to display goods and protect them from light-fingered customers. Salesclerks, urged to moderate their own acquisitiveness, were to encourage consumption on the part of shoppers while monitoring their honesty. This had the effect of pitting working-class employees, both salesclerks and store detectives, against middle-class customers. And stores heightened the double message by refusing to lay charges against well-connected wrongdoers in the interests of good customer relations.

The stores were aided in this maneuver by the cultural fiction of kleptomania, defined in the 1880s as a feminine disorder resulting from the natural processes of the female life cycle and the resulting emotional instability of women, leading them to pilfer goods. This definition was seized on by doctors, the stores, women themselves, and the general public as a means of saving middle-class respectability; such women deserved sympathy, not criminal charges. By using kleptomania as an explanation at the small cost of women's self-respect and the reinforcement of gender stereotypes, stores evaded responsibility for merchandising frankly geared to self-gratification;

doctors reinforced their claims to define women's natures; and married women were discouraged from examining the actual economic powerlessness that may have led them into the "kind of budget-stretching device" (p. 167) that shoplifting often represented.

Abelson's book is well put together and suggestive. It is a shame that she does not elaborate the implications of her findings. Is it significant that the stores' first pitch should have been toward women and children, by no means the most powerful members of society? Does this tell us something about the ways in which middle-class women as a subordinate group were essential to the development of consumer capitalism? Might the acceptance of women's tendencies to kleptomania have contributed to the gradual demise in the twentieth century of the notion of women's moral superiority? In its implication that women were all too vulnerable to the manipulation of merchandisers, Abelson's work stands in contrast to that of Susan Porter Benson, who in *Counter Cultures* (1986) assesses women customers as giving as good as they got in the ongoing struggle with department stores.

ANITA CLAIR FELLMAN
Old Dominion University

JUDITH SEALANDER. *Grand Plans: Business Progressivism and Social Change in Ohio's Miami Valley, 1890-1929*. Lexington: University Press of Kentucky. 1988. Pp. viii, 263. \$26.00.

Judith Sealander argues that business—as agent and issue—was at the heart of Progressivism. To prove this thesis, she uses Dayton, Ohio, to examine business involvement in four areas of Progressive activity: employee welfare, flood control, education, and manager-commission urban government. Sealander's approach has certain limitations. Neither "business" nor "Progressivism" is defined, making it difficult to determine their relationship, the role of other groups (such as professionals), or the relative importance of other issues, such as liquor, poverty, ethnicity, or families. Thus, the study ignores alternative explanations of Progressivism and even forgets about business as an issue. Nevertheless, this work is an instructive, sometimes intriguing story of forceful individuals, notable accomplishments, and belief in the compatibility of progress, efficiency, democracy, and "reform."

Sealander judiciously assesses the employee welfare policies of the National Cash Register Company. She cursorily explains president John Patterson's motives for establishing those policies, but she does show that his motives were not narrow self-interest. The discussion of flood relief and the creation of the conservancy district is even more interesting and carefully

nuanced, achieving just the right balance between praising accomplishments and condemning undemocratic methods.

The analysis of education is less satisfactory for several reasons. Sealander concludes that businessmen were necessary but not sufficient causes for the spread of Progressive education to public and private schools as well as to Antioch College. Antioch demonstrated Progressive educational ideas, but in location, intent, and impact it was a regional, not a local, institution. The author proves that business and Progressivism dominated private schooling in Dayton, but her treatment of public education is inconclusive on the extent and popularity of the reforms. The discussion of education is most flawed in its focus on the 1920s. Because the loose constellation of views making up "Progressivism" was shattered by 1920, and given the significant changes in the role and popular views of business, it is misleading to describe activities of businessmen in the 1920s as Progressive.

Dayton's association with city manager government is widely known. Beginning with Patterson's efforts in the 1890s, spurred by the organizing efforts of the Citizens Committee, and boosted by a disastrous flood, Dayton adopted a manager-commission form of government in 1913. This included nonpartisan at-large election of commissioners, civil service, the initiative and referendum, and departments of welfare and finance.

Sealander views this system as decidedly less democratic, but she believes the city's expanded welfare activities and its more efficient financial department were the primary motives and worth the price. The argument rests, however, on welfare services started during the decade, not on an assessment of costs or who was served. The author also fails to prove that these business leaders were motivated by a desire to help the less fortunate. Finally, to justify the increased city debt requires some analysis of how benefits were shared. These limitations gain in importance because they relate to an alternate explanation of governmental reform, that is, efforts to defeat Socialists and their proposals.

To some extent, the business-labor struggle haunts this study. Sealander notes that Dayton was home to the union-busting National Association of Manufacturers, but she does not consider the role or importance of those businessmen. Workers appear as recipients, and the powerful Socialist party is mentioned only briefly as reacting to business initiatives. Thus, the author shows the relative importance of some businessmen in parts of Dayton's public life, but she cannot demonstrate whether their actions were the product of cynical antilabor impulses or humanitarian benevolence. Such an analysis would have strengthened this useful book.

PHILIP R. VANDERMEER
Arizona State University

VICTOR B. HOWARD. *Religion and the Radical Republican Movement, 1860-1870*. Lexington: University Press of Kentucky. 1990. Pp. x, 297. \$32.00.

The announced purpose of this book is to examine the extent to which Protestant Christianity set the moral tone of the North from 1860 to 1870 by studying the interplay of religion and politics during the Civil War and early days of Reconstruction. An underlying assumption is that during this period, churches had the power to shape public opinion and influence the radical movement in the Republican party. The purpose is partially achieved. The assumption, although plausible, is not established with scholarly precision.

This book is an encyclopedic narrative of Protestant involvement in key moral and political issues during the Civil War era. In twelve chapters, Victor B. Howard provides clear accounts of how vigorously radical and liberal protestants supported the Thirteenth, Fourteenth, and Fifteenth amendments in addition to strong support for radical congressmen in the elections of 1860, 1862, and 1864 and how they supported Lincoln in the elections of 1860 and 1864, even though they were always critical of Lincoln for being too moderate.

Further, Howard provides interesting and adequate documentation showing to what extent and how radical Christians supported Radical Reconstruction and were involved in the politics of impeaching President Andrew Johnson. The book closes giving evidence of the churches' internal debates on universal or impartial suffrage and the content of the Fifteenth Amendment.

This is a valuable book inasmuch as it is a virtual encyclopedia of primary sources and secondary works covering the Civil War era, 1860-70. It is also valuable in its comprehensive treatment of a broad spectrum of Protestant figures and groups supplementing denominational studies and works on individual religious leaders of the time.

Its worthiness notwithstanding, there are problems with this study. First, the author gives the impression that this is a first of its kind. Indeed, the blurb on the jacket says, "This study shows for the first time how church and state interacted on the slavery question." In the text of the book, Howard shows no explicit awareness of a vast literature exploring precisely the subject matter of this book, even though he cites numerous secondary works in footnotes. Had the book begun by placing it in the context of similar scholarship, the focus would have been sharper and its value more apparent. There is no historiographical context at all.

The other major problem is that Howard too simply asserts that Protestant Christianity had a strong moral influence on the Radical Republican movement. There is no question at all that religion in this period was furiously engaged in politics, but there is little actual sociological or historical evidence

that there was in fact a strong influence. Although this thesis is plausible, it is a case yet to be made.

DONALD G. JONES
Drew University

ERIC L. HIRSCH. *Urban Revolt: Ethnic Politics in the Nineteenth-Century Chicago Labor Movement*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xvii, 253. \$39.95.

Eric L. Hirsch argues that ethnic identifications are most important for understanding and predicting the varieties of political behavior of the working class in Chicago during the nineteenth century. Hirsch, a sociologist, employs both historical and sociological methodologies to assert that "emergent, close-knit urban communities are often the social basis for the mobilization of urban political movements, especially revolutionary ones." These movements, however, are a product not of "a unified, class conscious working class" but of a "structurally isolated workplace and community networks and an oppositional culture" (pp. 202-03). Understanding the origins of workers' activism has long been an important and appropriate task of historians and sociologists. There is more to be done, and Chicago is a worthwhile subject of inquiry in this area. As a piece of interdisciplinary scholarship, Hirsch's study has potential but unfortunately falls short in some ways that are particularly troublesome for historians: sources, causality, definitions, and chronology.

The first two chapters of Hirsch's book trace the history of the labor movement from the end of the Civil War through the Haymarket bombing of 1886. Hirsch uses four sources in this discussion: Bessie Pierce's landmark study of the history of Chicago, Richard Schneirov's dissertation about the Knights of Labor in Chicago, and some of Philip Foner's work and local newspapers for additional information. This heavy reliance on just a few (although excellent) sources often renders the author's arguments weak and simplistic. For example, Hirsch claims that "the Knights were a backward-looking organization wishing to return to a competitive system of small producers and farm owners" (p. 55) without considering or citing Leon Fink's *Workingmen's Democracy: The Knights of Labor and American Politics* (1983). Hirsch misses any possible insights that a comparative perspective might bring through a discussion of the effects of unequal industrialization and the radicalization of the labor movement as a result of the entrance of German immigrants, both of which are discussed for New York City in Sean Wilentz's *Chants Democratic: New York City and the Rise of the American Working Class, 1788-1850* (1984).

It is in the middle three chapters, containing three case studies of urban, working-class groups defined by their ethnicity, the Anglo-Americans, the Irish, and the Germans, that the causality problems become

most apparent. Hirsch argues that ethnicity and the ethnically segmented Chicago labor market rather than skill, religion, language, earnings, date of immigration, or work were the most powerful determinants of a male worker's political identification. (Hirsch does not really discuss women at all in this book.) The Anglo-Americans were reformist because of their "belief in a hegemonic ideology that was borrowed from the middle class and articulated by the labor aristocrats in the various trades assemblies" (p. 115). Because of their historical situation with Great Britain, the Irish, primarily unskilled workers, chose nationalist organizations and machine politics. And the Germans, in the lower status trades, opted for radical, socialist, and anarchist labor organizations. This type of single-factor explanation is extremely unsatisfying in the light of the findings of recent works in labor, women's, and African-American history that reveal that people in the past, like people today, had multiple sources of loyalty and identification and that people within ethnic groups may be divided among themselves. (See, for a few examples, recent works by Roy Rosenzweig, Joshua Freeman, Gary Gerstle, Ronald Schatz, and Patricia Cooper for more sophisticated treatments of class, ethnicity, religion, and working-class organization.)

The book is marred by two definitional problems. First, and more minor, Hirsch consistently refers to the Protestant work ethic as the Puritan ethic, a small point, perhaps, but one that is jarring throughout the text. (Hirsch does not cite Daniel T. Rodgers, *The Work Ethic in Industrial America, 1850-1920* [1978].) Second, and more serious, Hirsch posits a narrow definition of working-class radical politics by assuming political allegiances and identification simply by membership and activity in a number of clearly defined radical (socialist and anarchist) organizations. David Montgomery, in two recent works (neither cited by Hirsch), has alerted labor historians to the radicalism of native-born Anglo-American workers on the shop floor through what would be considered "conservative" labor organizations.

Finally, Hirsch is not self-conscious enough about chronology, and this is first indicated in the book's subtitle. The study begins at the end of the Civil War and ends at 1886, so it is certainly not about the nineteenth century, which is a shame. Hirsch's story of nineteenth-century Chicago labor politics excludes the famous Pullman strike, Eugene V. Debs, and his railway workers. (Nick Salvatore's biography of Debs is not included in the cited works.) This is particularly disturbing, since the reformist, conservative tendencies of the railway workers (as gleaned from the journal *Railway Age*) are an essential part of Hirsch's argument.

In the last two chapters of the book, Hirsch considers the myriad of sociological theories that could account for his findings on ethnic-based labor politics in Chicago. On the one hand, historians will find the clear explanations of the various schools of thought,

both old and new, helpful. On the other hand, most historians will be uneasy with the lack of connection to the historical material. Hirsch, therefore, missed an opportunity to create a truly integrated, interdisciplinary work.

LISA M. FINE
Michigan State University

ROSS MILLER. *American Apocalypse: The Great Fire and the Myth of Chicago*. Chicago: University of Chicago Press. 1990. Pp. ix, 287. \$24.95.

Ross Miller appropriates Chicago's Great Fire of 1871 as a prism through which he critically assays its urban history over the ensuing twenty-two years, ending with the World's Columbian Exposition of 1893. Why and how Chicago's postfire history was reconstructed in noble chapters is the central focus of this book.

These streets are often walked, so the route and its bench marks are familiar. Hence, any author setting out anew must add something that is conceptually original to what we know. (Most recently we have books by Christine Miesner Rosen on the fire, James Gilbert on the fair, and an exhibition by Susan E. Hirsch and Robert I. Goler on Chicago in the 1890s; in progress are still more books, plumbing aspects of the city in the late nineteenth century, being prepared by Donald Miller, Karen Sawislak, and Carl M. Smith.)

By design this book intends to be provocative, even if little that its author covers is entirely new. It is a twin-fisted effort to confront prevailing scholarly interpretations. As such, it imposes on its readers to reconsider what they thought they knew. Although Miller's focus is too selective for scholars who know these chapters of the city's history intimately (thanks to Paul Angle, Emmett Dedmon, Perry Duis, and Bessie Louise Pierce), his book merits careful reading.

The author probes questions that cultural historians, borrowing from the newly replenished toolboxes of ethnography, folklore, and textual criticism, are now assessing with renewed intensity. Such a pursuit, admirably represented in a recent book by T. H. Breen on East Hampton, Long Island, explicates how and why a particular rendition of local history is configured, and reconfigured, in the face of specific sets of circumstances. Hence, at the center of Miller's study is a query: "Why was Chicago so successful in its self-conscious legendizing—a process that still continues?" (p. 3)

Miller draws from historical sources that are, for the most part, architectural and literary. Therefore, his insightful analysis, imbued with ironic and rhetorical sensibilities, largely reflects conceptions of Chicago as they are represented in the expressions of such well-known figures as Theodore Dreiser, Upton Sinclair, and John Wellborn Root. Understood on these self-selecting terms, this book is a shrewd and

opinionated reading of the city's history between 1871 and 1893.

Miller tries to color Chicago boldly. Although his choices of sources, imagery, and interpretation raise inevitable questions that require amplification and refinement, this book must be not ignored.

MICHAEL H. EBNER
Lake Forest College

LIZABETH COHEN. *Making a New Deal: Industrial Workers in Chicago, 1919–1939*. New York: Cambridge University Press. 1990. Pp. xviii, 526. \$27.95.

This book will be of interest to a wider audience than just labor historians. Students of ethnicity, mass culture, the urban experience, and American politics will all find something stimulating here. Elizabeth Cohen has woven an impressive variety of primary sources together with the existing rich scholarship on Chicago to produce a significant contribution to our understanding of U.S. history between the wars.

Cohen's central argument is clear: that rank-and-file workers "made" the New Deal, based on the ways working-class culture had changed during the 1920s and early 1930s; that they were motivated ideologically as well as practically, seeking to institute a "moral capitalism"; and that this "moral capitalism" rested on two new institutions, an interventionist state and the industrial unions of the Congress of Industrial Organizations (CIO).

Chapters 2 and 3 provide an analysis of the development of "mass culture" in the Chicago of the 1920s that is sure to influence the way scholars talk about "Americanization" from this point on. Cohen's presentation is insightful and richly detailed, as she explores ethnic self-help and fraternal organizations, churches, ethnic banks and savings and loan institutions, chain stores, radio, and movies. She offers this detail within a carefully constructed framework that emphasizes the activity of the "consumers" in making choices and in structuring their consumption of this new culture. Her argument highlights the changes wrought within these communities and also the resistance to homogenization.

Her presentation of the 1930s rests on the foundation she establishes here. The Great Depression furthered the deterioration of many ethnic institutions and their replacement by New Deal agencies and programs. But this very development was pushed forward, Cohen argues, by workers who saw themselves as part of a larger entity than their ethnic group, largely because of their participation in this new mass culture. Cohen also argues that, far from leading workers to see themselves as part of an amorphous middle class, this new mass culture actually enhanced a class consciousness among American workers.

Cohen realizes that this did not occur magically in the darkness of a movie theater or around a radio set.

She carefully explores other elements of this process, elements that both nurtured a sense of belonging to a separate class and limited its scope within the range of options provided by a continued capitalism. For her analysis, the welfare capitalism of the 1920s—designed by American industrialists—is the key element. It gave workers expectations and standards, she argues, that they later sought, amid the crisis of the depression, to have instituted and guaranteed by the government and new unions. Here Cohen finds the genesis of “moral capitalism”—her term for the thinking of rank-and-file workers in the 1930s.

Cohen's goal is to restore these workers to the center stage of discussions about the New Deal, a position she feels has been undermined by years of scholarship that have emphasized the roles played by Franklin Roosevelt, “corporate liberals,” and union leaders. To no small degree, she succeeds. Yet, in the end, by arguing that, for all intents and purposes, the rank and file got what they wanted out of the New Deal, the Democratic party, and the new industrial unions, she offers a rather deterministic picture, one that glides over most conflict and leaves the reader with a feeling that nothing else was possible. It is just a little too neat and tidy, particularly for a period as rambunctious and contentious as any in American history.

PETER RACHLEFF
Macalester College

STEPHEN H. NORWOOD. *Labor's Flaming Youth: Telephone Operators and Worker Militancy, 1878–1923*. (The Working Class in American History; Women in American History.) Urbana: University of Illinois Press. 1990. Pp. xii, 340. \$32.50.

This excellent book demonstrates both the continuing strengths and the interpretive limits of the “culturalist” approach to labor history that E. P. Thompson pioneered and that Herbert Gutman adapted to the industrializing United States. Stephen H. Norwood's protagonists are the young women who created, in the years surrounding World War I, the first (and, to date, the only) national union of telephone operators in North America. Most of these organizers were working-class Irish women from Boston. The contradiction between the educated, well-dressed image of the new occupation and a reality that combined the worst aspects of paternalism and authoritarianism spurred them to activism. Norwood's careful description of the “work experience”—a model of how such chapters should be written—establishes that operators had less control over the pace and conduct of their labor than did most unskilled factory hands. In the days before direct dialing, Bell System managers even “specified the exact enunciation of numbers” and required questions “to be posed with the rising inflection on the last word” (p. 35).

Norwood devotes most of his study to a thick

narrative of organizing campaigns, strikes, and hostile government intervention in the industry during and just after World War I. Despite the dates in the subtitle, the book really focuses on a single decade: from 1912, when Boston operators formed the first local with the help of the Women's Trade Union League (WTUL), to 1923, when an internally divisive struggle against the New England Telephone Company reduced what was then the self-governing Telephone Operators' Department of the International Brotherhood of Electrical Workers (IBEW) to an impotent shell. At its height in 1919, the union claimed eighteen thousand members in the United States and Canada, less than one-fifth of those eligible to join.

The author supplies all of the necessary organizational particulars of a complex story, but his major concern is the making of the trade union woman. As unmarried women in their late teens and early twenties, telephone operators brought an exuberant self-confidence to labor activism that Norwood credits for much of their success. They donned cosmetics and stylish clothes in defiance of both the company and the Catholic church. They attracted new members and pleased old ones with elaborate dances, parties, and bazaars. They flocked to workers' education classes taught by such instructors as Felix Frankfurter and Harold Laski. And, against strikebreakers and hostile employers, they employed ridicule and refashioned lyrics to popular songs (“Yes we have no phone numbers, We have no phone numbers today” [p. 287]) and even, on occasion, used their fists.

Norwood's own grandmother was a charter member of the Boston local, and her memories and those of her aged comrades give these sections much of their vigor and verisimilitude. Along with such historians as Dorothy Sue Cobble, Kathy Peiss, and Jacquelyn Hall, Norwood convincingly shows that the sensual habits of the “new woman” often helped to motivate working-class “flappers” to join unions instead of ignoring them in the rush to consume.

Norwood is less persuasive, however, in explaining why the operator's union enjoyed only a few brief years of triumph. He does include details of factional struggles, the resentment of IBEW officials and craftsmen, and American Telephone and Telegraph's introduction of the job-eliminating dial system. But he barely touches on matters of at least equal importance, such as the fact that few operators regarded the occupation as a permanent one and that strikebreaking college students and society women seemed to have been able to perform the job effectively. The author also does not make clear why the Telephone Operators' Department was so restricted geographically. Strong, Irish-led labor movements and active WTUL chapters also existed in New York and Chicago during this period. Yet, apart from Butte, Montana, the operators' union was never able to build strong, employer-recognized locals outside of the Boston area.

By concentrating on what E. P. Thompson called the "heroic culture" of working people, Norwood helps to deepen our understanding of both the history of wage-earning women and the vital diversity of the American Federation of Labor. But he slights the sources of weakness that ultimately proved the operators' undoing.

MICHAEL KAZIN
American University

STEPHEN J. OCHS. *Desegregating the Altar: The Josephites and the Struggle for Black Priests, 1871–1960*. Baton Rouge: Louisiana State University Press. 1990. Pp. xviii, 500. \$39.95.

Stephen J. Ochs provides an excellent example of the new Catholic history that presents a critical and often censorious view of the Catholic past. Specifically, Ochs sets out to explain why the Roman Catholic church in America produced only a negligible number of black priests before the 1930s, thereby robbing black Catholics of leadership roles in the church. Ochs approaches his topic through a case study of the St. Joseph Society of the Sacred Heart, or the Josephites, the only Catholic religious order devoted exclusively to the evangelization of African Americans. In 1871 four Mill Hill priests, or English Josephites, arrived in Baltimore to begin their mission among blacks. These missionaries faced fierce opposition from a Catholic hierarchy that viewed blacks as mentally and morally deficient. Because of such attitudes, only three light-skinned American mulattoes received holy orders before 1886, and they were ordained abroad.

Nevertheless, John Slattery, a flamboyant Irish American who became the first superior general of the American Josephites in 1893, integrated the Josephite college and seminary in Maryland and started three blacks successfully on their way to ordination. But Slattery's dream was dashed on the rocks of societal and ecclesiastical racism. The explosive opposition to his plan helped drive Slattery out of the church in 1904. His apostasy nearly destroyed the Josephites. Not until World War II did the Josephites reclaim Slattery's quest for a black clergy.

Meanwhile, the Society of the Divine Word, responding to papal pressure, opened a segregated seminary for blacks in Mississippi in the early 1920s. In addition, Thomas W. Turner, a militant black professor who began to organize black Catholics during World War I, applied pressure on the hierarchy to foster priestly vocations among blacks. Eventually, the liberalizing impact of the New Deal, World War II, and various Catholic interracialists created a climate for racial change. Several religious orders and some bishops then began to open their college and seminary doors to blacks. By 1945 twenty-one blacks wore clerical robes, up from two in 1933. By 1960 the number of African-American priests had increased to

108. Finally, between 1968 and 1989 thirteen blacks joined the ranks of the American episcopacy.

Ochs ably analyzes these developments and many more in this wide-ranging and magnificently researched book. Its judgments are finely balanced and nuanced. Although the copious detail that Ochs marshals to penetrate the labyrinthine bureaucracy of the Catholic church may make the book somewhat daunting to the general reader, it will be an absolute treasure to scholars in the field of Catholic social history and black and white relations. By sharply analyzing the corrosive legacy of institutional racism, Ochs gives the reader an insight into the revolt of the black priest George Stallings, Jr., and his establishment in 1989 of an independent Catholic African-American congregation in defiance of the archbishop of Washington, D.C. Indeed, this book leaves one wondering why there were not more priests like George Stallings in the American past.

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JAMES C. JUHNKE. *Vision, Doctrine, War: Mennonite Identity and Organization in America, 1890–1930*. (The Mennonite Experience in America, number 3.) Scottdale, Pa.: Herald. 1989. Pp. 393. \$19.95.

The new social historians have reawakened the profession to the importance of gender, ethnicity, and social class. Yet, in so doing, they have often slighted the theme of religion. In this study of the American Mennonites, however, James C. Juhnke helps redress the imbalance. Juhnke maintains that a vigorous Anabaptist heritage, plus a plethora of newly created church institutions, marked out the boundaries of life for this important American subculture.

Juhnke sets forth the complex Mennonite world (including both the Amish and the Hutterites) with great skill. Although they numbered less than one hundred thousand in 1910, the Mennonite family had fragmented into a bewildering variety of distinct organizations: "Old" Mennonites, Reformed Mennonites, Old Order Mennonites, Old Order Amish, General Conference Mennonites, Amish Mennonites, Mennonite Brethren, Hutterian Brethren, and so on. These groups disputed vigorously among themselves over such issues as the ban (excommunication), avoidance (shunning of the banned), communal living arrangements, and regulations regarding dress, telephones, and the use of automobiles (one group compromised and painted their bumpers black). We tend to smile at such fancy footwork, but the issue at stake proved serious indeed: where does one draw the line between "the church" and "the world"?

Influenced by contemporary conservative evangelicals, turn-of-the-century Mennonites made a variety of accommodations toward entering mainstream

Protestant life. Then, as Juhnke notes in an especially fine chapter, the American nation drew the line for them. With the country's entrance into World War I, these largely rural people found themselves harassed over both their "Germanness" and their historic pacifism. Arson, mob violence, and physical threats dogged Mennonite communities, usually over the issue of war bonds; harassment and imprisonment affected a number of Anabaptist conscripts.

The war, however, also renewed the Mennonite consciousness of themselves as a "distinctive people"—they refused to bear arms. Drawing on the organizational skills that they had forged during the prewar years when they had established numerous missions, schools, and hospitals, the postwar groups created a Mennonite central committee, an inter-Mennonite relief effort that soon stretched around the globe.

Organized relief came to be seen as a contemporary reflection of their historic biblical faith. In addition, during the early 1930s the Mennonites began to cooperate with the much more liberal Quakers and the Church of the Brethren—"the historic peace churches"—over the issue of conscientious objection to war. Thus, the post-World War I Mennonites began a renewed quest to maintain their identity as "God's people in mission to the world" (p. 314).

With clarity and balance, the author has shown how the various parts of the Mennonite "mosaic" confronted modern American life yet retained their historic faith perspective. Juhnke has written a first-rate social history of a religious "family" whose influence has extended far beyond its actual numbers.

FERENC M. SZASZ

University of New Mexico

ARNOLD SPARR. *To Promote, Defend, and Redeem: The Catholic Literary Revival and the Cultural Transformation of American Catholicism, 1920–1960*. (Contributions to the Study of Religion, number 25.) New York: Greenwood. 1990. Pp. xvi, 221. \$39.95.

The Catholic literary revival was an important component of the broader transformation of American Catholic culture between the 1920s and 1950s, but until now the names associated with that revival were inevitably European: Georges Bernanos, Hilaire Belloc, Paul Claudel, François Mauriac, and many others. Arnold Sparr acknowledges the European sources of Catholic literary life in America, but this book will be valued for its rediscovery of important American proponents of the revival.

Sparr argues that certain Catholic intellectuals in the late 1920s—prompted in part by the Al Smith debacle and other public controversies—sought to promote the status of their church while redeeming secular society from fragmentation and aimlessness. Influential male clerics such as the Jesuits Francis X. Talbot of *America* and Daniel Lord functioned as

impresarios of Catholic literary productivity, compiling lists of approved books and authors and staging plays and pageants in vigorous defense of the faith. Lord's directorship of a national Catholic youth society, the Sodality of the Blessed Virgin, provided a link to the vast Catholic Action network of special interest groups working to advance the social and spiritual goals of the hierarchy.

The explosion of Catholic activity in the 1930s and 1940s has been treated before from a variety of angles. Sparr distances himself from the provocative interpretation of William Halsey, who argued that the seeming vitality of Catholic thought and action in the period reflected an effort to restore that genteel American optimism long abandoned by secular intellectuals, a rear guard action that masked an inability to confront "the warp and woof" of experience head-on. Sparr maintains that the literary revival was an aggressive, creedal-driven affirmation of practical Catholicity more than a response to status concerns. He makes a strong argument that the Neo-Scholastic philosophy that energized the movement was more dynamic than Halsey and others have allowed, finding particularly in the work of University of Notre Dame English professor Frank O'Malley a Christian existentialism inspired by Jacques Maritain and other Catholic refugees from totalitarian Europe.

Yet, by Sparr's own admission, the actual literary work produced by the revival's American branch was, through most of these years, derivative and unmemorable: sentimentalizing, moralistic fiction and triumphalistic criticism. Only in the mid-1940s did Catholic American writers begin to "work out their own idiom despite the advice of critics" (p. 147). Sparr's treatment of such forgotten novelists as Harry Sylvester and Joseph Dever is one of the book's strengths. These authors and others like them—including J. F. Powers—struggled to create religious fiction true to their immigrant backgrounds, even if this meant straying from the lofty prescriptions of the Neo-Scholastics. But the literary revival ended without giving birth to the "Great American Catholic Novel," a victim of the desire of postwar Catholic artists and intellectuals to be taken seriously by their secular counterparts.

This book is a fine study of "official" Catholic culture in the years of its greatest self-conscious estrangement from a secular establishment that was never so monolithic as it appeared from the outside. But Sparr's narrow definition of "Catholic" causes him to ignore large questions about the construction of authorial identities. In the 1920s, F. Scott Fitzgerald accepted the judgment of Catholic tastemakers that his was the work of an apostate. Thirty years later Jack Kerouac firmly staked his claim as a Catholic writer despite superficial evidence to the contrary. Sparr's focus on professionally Catholic writers diverts his attention from powerful ethnoreligious un-

dercurrents finally brought to the surface by a rich diversity of Catholic American voices.

JAMES T. FISHER
Yale University

PETER L. JAKAB. *Visions of a Flying Machine: The Wright Brothers and the Process of Invention*. (Smithsonian History of Aviation Series.) Washington, D.C.: Smithsonian Institution. 1990. Pp. xviii, 263. \$22.50.

There have been many biographical studies of the Wright brothers and some that have analyzed specifics of the airplane designs. But Peter L. Jakab has written a fascinating—and, in its own way, original—analysis of the step-by-step procedures practiced by the Wrights in developing their pioneering aircraft. In Jakab's own phrase, he has analyzed their "process of invention."

Jakab includes enough biographical and family background to enable readers to understand the inquisitive attitudes and close family relationships that shaped the brothers' approach to their quest of airplane flight. He also gives a brief, technical survey of contemporaneous civil engineering and aeronautical experimentation by Otto Lilienthal, Samuel Langley, and others to provide an effective backdrop in understanding the Wrights's approach to a series of engineering problems. There had been considerable experimentation directed toward successful winged and powered flight, but much of it proved to have subtle flaws. One of Jakab's chief contributions is his thorough description of the Wright brothers' procedures as they fashioned simple but ingenious test equipment that uncovered many deficiencies and gave them the information to design more effective components.

Their wind tunnel was invaluable. Typically, they built more than one; even in experimental equipment, they seemed to follow a consistent pattern of incremental development and testing. Jakab assisted in preparing and operating a replica tunnel at the National Air and Space Museum in the 1980s, where the Wrights's original aerodynamic shapes for wings were carefully tested again. "It was possible thereby to get a sense of the procedures and problems associated with running the tests not always apparent from the literary evidence," Jakab writes (p. 127). Throughout this carefully reasoned study, Jakab's attention to such detail is convincingly evident. Not only is he able to construct enlightening scenarios that inform the reader of what the Wrights accomplished but, more important, he is able to explain how and why the brothers proceeded as they did.

Jakab stresses the evident ability of the Wright brothers to apply "visual thinking," the knack of visualizing how scientific data would result in actual design concepts. Their apparatus for measuring actual lift and drag involved their refined wind tunnel

as well as equipment to test wing shapes by means of a balance device. The test shapes and balances were made of bicycle spokes and hacksaw blades, Jakab explains, but were tested and analyzed with considerable mathematical sophistication. Based on the physical readings from their contrivance, the Wrights used geometry to prepare schematics of aerodynamic forces—visual thinking that led to skillfully designed wings, shaped and strengthened to handle the job effectively.

There are many other examples, such as the fabrication of vastly more efficient propellers. Similarly, Jakab notes that the engine, actually somewhat crude for its time, was conceived in a context of relationships between it and the realities of lift, drag, and velocity of the plane in the air. Those ideas are common sense now perhaps but not when the Wrights were pioneering frontiers of flight.

All in all, this is a knowledgeable book, rich in detail and skillfully written. It is a valuable addition to our understanding of the Wright brothers and to the process of invention and engineering.

ROGER E. BILSTEIN
University of Houston,
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PAULA S. FASS. *Outside In: Minorities and the Transformation of American Education*. New York: Oxford University Press. 1989. Pp. x, 308. \$24.95.

At a time when debates on diversity in the classroom, the place of multiculturalism in the curriculum, and the preservation of a common political heritage are exploding in the media and in academic journals, we would do well to realize that, as Paula S. Fass's study so nicely shows, American educators have been struggling with these issues, in one form or another, throughout the twentieth century. In her sweeping study of mass education, Fass focuses on the efforts of reformers to balance what she terms the ecclesiastical goals of mass schooling—to integrate and unify the nation, with a liberal commitment that democratic education must meet the needs of individuals and, increasingly, the needs of different groups of Americans.

The first half of the study deals with mass immigration in the early twentieth century and the growth of public schooling, particularly secondary education. Scholars of Progressivism and the history of education will be familiar with much of this discussion on the attempts to make school curricula relevant to foreign populations. Fass provides an important perspective, however, by reminding us that, though sometimes incomplete and contradictory, the best reforms struggled with an appreciation of diversity as well a commitment to schooling as a unifying force. Fass points out that visionaries such as John Dewey never believed that the school alone, could be an

adequate vehicle for reform. The triumph of IQ tracking hardly coincided with Dewey's vision of meeting the needs of the malleable child.

To evaluate the extent to which high schools by the 1930s and 1940s integrated ethnic groups into mainstream education, Fass analyzes ethnic participation in extracurricular activities, which became so important to the modern high school. Using yearbooks, she found that, in New York City, ethnic background determined patterns of participation, even in the second generation. Jews, for example, participated disproportionately in academically oriented activities (especially in schools with large Jewish populations) and Irish males in student government.

The second part of the book deals with other "outsiders" who dominated reformers' concerns by mid-century. Fass shows how New Deal relief, rather than educational policy, helped blacks by providing economic resources that enabled many more to complete high school. She also looks at massive and rather successful efforts to deal with black illiteracy among army draftees in World War II. A discussion of women in higher education follows; although many of the basic issues concerning the crisis of women and education have been dealt with elsewhere, Fass nicely details how Progressive concerns to make college curricula relevant to the changing needs of students merged with traditional expectations about women's roles. A final chapter provides an excellent and needed discussion of how Catholic education stood as an alternative to, and was influenced by, liberal education.

The book is more a series of articles, connected by similar concerns, than a broad narrative of twentieth-century mass education or a detailed analysis of any one issue. The discussion of blacks and depression relief seems rather unconnected to the previous discussion of immigrants; thus, comparisons are hard to make. There is a connection between the increasing numbers of ethnics in American high schools and New Deal efforts with respect to blacks, which is not addressed, that had to do with the efforts of state and federal officials to keep poor children, black and ethnic, out of the labor market. Some of the problems facing the diverse high school were a consequence of the fact that, by mid-century, high schools had become custodial institutions for many rather than training grounds for skilled employment. Finally, the focus on women and higher education rather than a sustained discussion of women and secondary education is startling, given the earlier emphasis. Nevertheless, this is a stimulating book for those interested in the role of education in the history of American liberal reform and will be very valuable for courses in the social history of twentieth-century education.

MIRIAM COHEN
Vassar College

ERASMO GAMBOA. *Mexican Labor and World War II: Braceros in the Pacific Northwest, 1942-1947*. Austin: University of Texas Press. 1990. Pp. xiv, 178. \$25.00.

In August 1942, the United States and Mexico reached agreement on a guest worker program to meet the wartime needs of U.S. agriculture. By 1946, over two hundred twenty thousand braceros, or "helping hands," had entered the United States, most to work in California and the Southwest. A fifth of that number, however, found themselves in the Pacific Northwest, for Mexicans a more unusual setting. This volume is an account of that experience. Concise, clear, and largely traditional in approach and sources, the book begins with a neat survey of the sociology of agriculture in the Northwest, then moves on to explore the development of government policy amid the farm labor crisis of World War II. The bulk of the book then details northwestern growers' use of the program and the conditions of life and labor for the guest workers.

Erasmus Gamboa argues that braceros in the Northwest faced an unusual combination of inhospitable conditions, beginning with an unfamiliar climate and an equally unfamiliar host population. In addition, the guarantees built into the bilateral agreement—that workers would earn prevailing wages, be adequately housed and fed, and suffer no prejudice—were vigorously subverted by northwestern growers intent on reducing labor costs. Growers, he points out in some of the book's most interesting passages, were ambivalent about the program. Convinced that they desperately needed this government-assisted supply of foreign workers, they championed the program in theory while undermining it in practice. Resenting government "interference" and largely free of supervision because of their remote location, northwestern growers flagrantly violated the law's standards on wages and living conditions.

Gamboa shows that these actions led in turn to greater worker resistance in the region, including frequent strikes and a very high desertion rate. Northwestern braceros, he maintains, had fewer options than those elsewhere. Far from supportive communities, from government inspectors, or from Mexican consulates, they had no one to depend on but themselves. All of these issues are carefully handled. Without undue theatrics, Gamboa builds a case that the program was a failure: "in practice the binational agreement was a worthless promise between two nations because it allowed employers ultimate say over the imported men" (p. 73). Thus, when a new bracero agreement was negotiated in 1947, northwestern farm groups declined to participate, partly because the new terms were more costly but also because the old program had proved difficult for all involved.

This volume is good as far as it goes, but it is not the definitive study that it might have been. By relying

mostly on government documents, Gamboa limits the scope of analysis. A thin chapter entitled "Bracero Social Life" is all that is possible without sources that speak for the workers themselves. Then, too, a more ambitious book would have sought a wider historical context and pushed on to investigate the relationships between braceros and the growing Mexican-American population of the region.

JAMES N. GREGORY
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JAMES N. GREGORY. *American Exodus: The Dust Bowl Migration and Okie Culture in California*. New York: Oxford University Press. 1989. Pp. xviii, 338. \$24.95.

Five decades after the publication of John Steinbeck's *Grapes of Wrath*, James N. Gregory has in his book moved beyond the conjured images of powerful fiction to a fuller understanding of this "heartland diaspora." The study of Dust Bowl migration during the 1930s and 1940s leads Gregory to challenge some long-held stereotypes. Steinbeck's Joads were not typical, he contends. Exodusters were not destitute dirt farmers; 57 percent of them came from cities and towns in Oklahoma, Texas, Arkansas, and Missouri. Seventeen percent were white-collar workers or professionals.

Gregory's data also suggest that this massive outmigration spanned three distinct eras, beginning as early as 1910. He also concludes that each removal was a response to a different imperative. The population shift that occurred in the 1930s, for example, was depression-driven by migrants affected by declines in construction, transportation, and oil production. Those who were tenant farmers, while beset by dust and drought, were much more adversely affected by the New Deal's Agricultural Adjustment Administration, which rewarded landowners for taking acreage out of production. Gregory further contends that the transit to California was relatively easy, permitting frequent returns. The sojourner nature of this settlement as well as its characteristic chain migration suggests a previously unrecognized parallel with immigration to the United States.

The second part of this well-documented study traces the development of California's distinctive "Okie subculture," ranging from Los Angeles County northward to the San Joaquin Valley. Gregory examines the vertical as well as the geographic mobility of the Okies, while tracing the impact of their "plain-folk" American values on religion, music, and the state's politics.

Although the California newcomers pursued the goal of assimilation, discrimination, both overt and covert, led them also to pursue strategies of avoidance. Okies minimized external contacts by affiliating with familiar or welcoming institutions. Among these

were several Protestant congregations, particularly the Pentecostals. This embrace of evangelical religion assumes significance in light of a recent claim by religious historian Sandra Frankiel that well into this century Protestant evangelicalism had not yet established a hegemony in California.

The important role of music in Pentecostal liturgy was especially appealing to the newly arrived heartlanders, for music, particularly of the country genre, occupied a prominent place in their lives. Gregory's recreation of this facet of Okie life is rich with references to Woody Guthrie, Merle Haggard, and Gene Autry, the popularity of each having been enhanced by Southern California's emerging media.

Gregory supports the contention that the Okie subculture in California was a politically liberal outgrowth of the heartland's neopopulism and "resurrected radicalism" (p. 142). Predictably, it was not welcomed by the state's Republican leadership who perceived the influx in terms of opposition votes. The study reminds us that the newcomers did indeed have a long-term political impact, most dramatically reflected in the liberal legislative legacy initiated by longtime assembly speaker Jesse Unruh.

It is interesting that in the examination of political life, Gregory does not refer to the fact that Oklahoma had the largest national concentration of agrarian socialists. This affiliation could help explain the migrants' liberal tradition as well as their often sympathetic relationship to the Workers Alliance and the United Cannery, Agricultural, Packing, and Allied Workers of America.

A second concern arises from Gregory's designation of the Dust Bowl region as the Southwest, a term printed alternatively in upper and lower cases, suggesting the author's own uncertainty about the Dust Bowl's designation as region or location. The confusion is compounded by the fact that, since the late nineteenth century, Southwest has been the accepted designation of the region that includes Arizona, Nevada, and New Mexico.

A final concern is the author's determination that California history actually begins with the gold rush, thus dismissing the earlier Hispanic period whose heritage is still vividly reflected in Southern California's place names, townships, and architecture. In another instance Los Angeles before 1930 is described as a sleepy village, thus ignoring the effect of the real estate booms of the 1880s and 1920s and the fact that by 1909 Angelenos owned more cars per capita than residents of any other city in the world. The portrayal also ignores the fact that in the 1920s Southern California led the nation in oil, citrus, and motion picture production.

But regional cavils aside, this book is an important contribution to twentieth-century social history. It combines advanced demographic analysis with an effective use of public records to create a more

accurate picture of one of the most important population movements in recent American history.

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Pomona

DAVID GLASSBERG. *American Historical Pageantry: The Uses of Tradition in the Early Twentieth Century*. Chapel Hill: University of North Carolina Press. 1990. Pp. xvii, 381. Cloth \$45.00, paper \$14.95.

This book traces the rise and fall of an art form that thrived only in the second decade of the twentieth century. Historical pageantry now appears so bizarre that it is hard to understand who would want to look at it and why it was taken seriously. David Glassberg's judicious history deserves praise first of all for carefully unpacking the cultural and social meanings of this lost form.

Historical pageants were elaborate celebrations of localities. They characteristically strung together a series of scenes that traced the "history" of the town. The arrival of whites, the "inevitable" dispersal of American Indians, the tilling of the soil, and the coming of industrial society were the sorts of things portrayed. The pageantry movement was brought over from England around 1910. In the next decade, dozens of pageants were put on in small towns and big cities. St. Louis spent \$125,000 for its pageant in May 1914, about the same amount D. W. Griffith used to make *Birth of a Nation* (1915). Over three hundred thousand people came to watch that four-night festival. It had literally a cast of thousands.

Pageants were acts of civic reconciliation, although of a specific and limited sort. They were meant to create a sense of community among white Anglo-Saxon Protestants and immigrants, thus echoing dozens of other turn-of-the-century middle-class reforms. Celebrations of the local past would help steer a unified town into the future. Although the image was of fraternity, not everyone was included. Anglo-Saxon Protestants shared the stage with recent immigrants, but African Americans and labor unions were excluded. (American Indians were generally represented by whites.)

Glassberg is insightful in explaining who put on pageants. In the years before World War I, recreational social workers, old Anglo-Saxon elites, and theater people aligned to produce the dramas. But by the 1920s all were moving in other directions. Dramatists gave up on bringing art to the people. Anglo-Saxon elites turned their attention to historic preservation and museum work. And the social workers turned to more manageable projects, paying less attention to grand historical pageantry. Pageants continued to be produced until the 1940s, but already by the 1920s they were lifeless and stale.

One of the author's primary interests is to connect pageantry to changing conceptions of history, and

here he produces some of the best analysis in the book. Back in the 1870s, Glassberg tells us, Fourth of July oratory was loaded with images of a stable and unified past leading seamlessly into the present. Anglo-Saxons were the sole participants in this history. But, at the close of the century, both the coming of immigrants to cities and the economic difficulties of small towns created problems for the celebratory prose. Pageants filled the gap, explaining why there might be tension but also suggesting a happy resolution. Glassberg makes a number of intriguing and pointed comparisons between pageantry and the new history of James Harvey Robinson and Charles Beard.

At times the author rather awkwardly hauls in giant background forces to explain pageantry. According to Glassberg, industrialization and immigration undermined town autonomy, which led to the need for harmony and, hence, the pageants. But threatening immigrants had been coming to the United States since at least the 1830s, and towns had been falling prey to the ravages of industrialization from the same time. Why pageantry became so attractive around 1910 needs more precise explanation.

This complaint, however, does not detract significantly from the book's virtues. Glassberg's analyses of the pageants are wonderful, complex, and well thought-out. And his insights into the popular uses of history complements nicely the work done on this subject by scholars such as Warren Susman, Susan Porter Benson, and Karel Ann Marling.

KENNETH CMIEL
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LAWRENCE J. FRIEDMAN. *Menninger: The Family and the Clinic*. New York: Alfred A. Knopf. 1990. Pp. xix, 472. \$29.95.

Lawrence J. Friedman's book is a major departure for its author. Friedman's three previous books primarily explored aspects of nineteenth-century intellectual and cultural history. This volume is a study of a twentieth-century Kansas family and their world-famous psychiatric institution. Grounded in extensive archival research and oral history, this book is, in fact, the first study based on "full and unlimited access" to Menninger archives. As in his previous books, Friedman is earnest, wide-ranging, and provocative; he is also a bit heavy-handed and fixated on psychohistorical interpretation.

Friedman divides the career of the Menninger institution into three major phases: "a family spirit," 1919–45; the making of a national institution, 1941–53; and the passing of the founding generation. His two principal claims are that the Menningers were unsure they wanted to expand their influence much beyond their specialized therapeutic sphere until the post-World War II period and that family dynamics exerted an enormous influence on institutional devel-

opment during all three phases, especially in the first two. Indeed, in contrast to the Mayo Clinic and other family-originated, later national institutions, familial influences “never entirely dissipated” at Menninger, not even in the last phase of its history.

Friedman pursues his family dynamics theme in considerable detail. At the core of his interpretation is the identification of a system of “family alliances.” The original Dr. Charles Menninger formed an “alliance” with his youngest son Will, but this alliance was subordinate to the principal one formed between Dr. Menninger’s deeply troubled yet strongwilled wife Flo and her first born son Karl. Working this system, Karl emerged as the dominant member of the family and was thus able to shape the “family business” into a psychiatric partnership. He was also able to maneuver both his father and brother into secondary, largely administrative roles while he pursued his own psychoanalytic and literary interests. Yet Karl felt unduly dependent on his mother and trapped in an unhappy marriage that played out derivative dynamics. Only when he found the strength through psychoanalysis to divorce his wife over his mother’s objections was Karl able to conceive of the Menninger mission on an ambitious national rather than local scale. Flo Menninger’s death in 1945 further accelerated the transformation of the Menninger institution.

There is a lot more of this psychohistorical interpretation, but, to his credit, Friedman makes valiant efforts to link the history of this family-dominated institution to larger developments in American psychiatry. As an example, he connects tensions between Karl and Will in the 1920s and early 1930s to Will’s initial preference for biological as opposed to analytic-dynamic psychiatry. Friedman links the expansionist phase of Menninger history in the period after World War II to entrepreneurial possibilities presented by the Veterans Administration and the rapidly developing National Institute for Mental Health. Connections, political skills, and opportunism, not just the redirected energy of family dynamics, served the Menningers well, as they did other national leaders in postwar psychiatry. Also, Friedman explains the passing of Menninger eminence in the late 1950s and early 1960s in terms that go beyond involuted family relationships and include the decline of psychodynamic perspectives (to which the Menningers remained committed) and the rise of biological approaches in the profession at large.

These commendable efforts to widen the context, however, are never entirely successful. Friedman admits that his first draft focused too narrowly on “in-house details” and that he tried to enlarge the scope of discussion in subsequent drafts. He distanced himself from the Menninger campus, read widely in the secondary literature of a field with which he was not completely familiar, and consulted leading historians of American psychiatry. Yet Friedman never quite seems to achieve his goal of writing

a national—as opposed to merely a local—history comparable to Gerald Grob’s account of the Worcester State Hospital. His links to national trends and events often seem glued on and incompletely integrated; his most common interpretive maneuver is to return yet one more time to family dynamics. Frequent repetition of the central theme, in this case, serves mainly as a deflection from deeper analysis rather than an occasion for integration and synthesis.

There are other failings, too. Friedman misses opportunities to study in detail Karl’s intellectual relationships with early contemporary psychoanalysts, such as Smith Ely Jelliffe, an indigenous American pioneer, and Franz Alexander, an influential émigré. Learning that Karl had an analytic session or two with Jelliffe in the 1920s and a more extended, formal analysis with Alexander in the early 1930s is interesting but does not substitute for the close examination of intellectual affinities and differences. In general, Friedman does not satisfactorily explore Karl’s intellectual endeavors. True, he offers synopses of the principal books and informs us that Karl felt “cut to ribbons” in a brief direct encounter with psychoanalyst and Freud biographer Ernest Jones. But he nowhere pursues a probing analysis of what Karl actually believed and why, of how contemporaries in the psychoanalytic community and elsewhere appraised his contributions, and of why he felt, perhaps appropriately, shredded by Jones. Will’s career is also incompletely treated. For example, Friedman mentions Will’s work with the army during World War II and alludes to his extraordinary rise to the rank of brigadier general—the highest ever achieved by a psychiatrist in the armed services—but he supplies no account of Will’s actual war work or of his personal assessment of what the work implied for civilian application. Nor does he adequately treat either of the Menningers’ postwar involvement with the movement for community-based psychiatric activism. Finally, Friedman fails to convey a real sense of the life of the Menninger institution from the patient’s perspective or even from that of the rank-and-file staff. We develop little feel for the place on a day-to-day basis and therefore no satisfactory way of comparing it to other private psychiatric institutions, university-run facilities, or state hospitals.

Overall, this work seems somehow to fall flat. It covers considerable ground but over large patches does so relatively superficially, leaving in its trail scores of unasked and unanswered questions. For all of its strengths, Friedman’s fourth book emerges as a competent, admirably researched, and clearly written local history that misses becoming one of those vivid, insight-generating case studies that illumines or perhaps even rearranges familiar patterns of interpretation.

THEODORE M. BROWN
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MICHELE HILMES. *Hollywood and Broadcasting: From Radio to Cable*. (Illinois Studies in Communications.) Urbana: University of Illinois Press. 1990. Pp. 221. \$24.95.

Historically, broadcasting in America has developed primarily through private investment. Even with periods of extensive regulation by the federal government, broadcasting in this country has had considerably more freedom than in most other parts of the world. Michele Hilmes has produced an excellent introduction to a most important historical subject. This is an invaluable work for both scholars and students that places film, radio, and television within the context of the national cultural experience.

In recent years technology has broadened the opportunities for television broadcasting by means of communication satellites and cable systems. Other technological developments, particularly recording and playback devices, have brought about even more changes in broadcasting. All of these new technologies undermine the concept of scarcity of product. The traditional giants of the broadcasting industry, namely, the radio and television networks and the film companies, today face these emerging technologies and their potential to fragment audiences.

This study explores the influence that film and television have exerted on each other. These two powerful means of communication have often been examined by scholars, but Hilmes documents "the considerable, although neglected, influence that Hollywood exerted—and continues to exert—over the development of the broadcast medium" (p. 1). The author correctly asserts that the film industry has played a central role in the economic evolution in broadcasting (including program forms and patterns of distribution) since the 1920s. Hollywood has been at the same time broadcasting's chief rival and main benefactor. The movie industry is the only other economic force powerful enough to balance broadcast interests. The book begins with a history of radio and describes the struggle between film and broadcasting interests over the right to define and control new technology. Later chapters examine the early years of television and the changing relationship between film and broadcasting brought about by cable, satellite, and pay television.

The book is essentially for academics or others with an interest in the history of broadcasting. Some readers may think the discussion too technical, but Hilmes is deserving of praise for a thoughtful and scholarly treatment of a difficult subject.

Overall, this is a well-written, interesting, even provocative book. It fulfills its obligation by providing a thorough and balanced evaluation of the sources, especially the rich secondary literature on the subject. There are some irritating spelling errors of the names of some personalities, such as Rudy Vallee and Ronald Colman, but not enough to detract seriously from the basic contribution of the book.

Because film and television play such central roles in our national culture, it is important to understand the historical processes that shaped them. Hilmes discusses the tensions between them: "public interest versus private profits, government regulation versus laissez-faire economics, free audience choice versus audience manipulation, the 'marketplace of ideas' versus the power of vested interest" (p. 5). The invaluable contribution of this study is its investigation of the historical interaction between the film and broadcasting industries, which other scholars have overlooked. The author has given us a lasting addition to creative historical research.

ARTHUR F. MCCLURE
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WILLIAM BODDY. *Fifties Television: The Industry and Its Critics*. (Illinois Studies in Communications.) Urbana: University of Illinois Press. 1990. Pp. x, 294. \$29.95.

William Boddy defines his study as "both a reconsideration of the critical consensus surrounding television's Golden Age and a historical analysis of the economic and regulatory ground upon which such programming briefly flourished in the 1950s" (p. 5). In this ambitious undertaking, Boddy succeeds admirably, delineating the growth of network power over all aspects of the television industry. Concerning other aspects of his objective he is not, however, equally successful.

Boddy's extensive research traces the intricately interwoven forces that contributed to network monopolization of American television programming. According to Boddy, the three national networks deliberately pursued parallel strategies securing their control of the industry. Through aggressively exploited sympathetic governmental regulation, the increasing substitution of Hollywood telefilms for the earlier pattern of New York-based live productions, shifting configurations of sponsors and sponsorship strategies, alterations in program procurement and scheduling, and the frailty of local affiliates, the networks achieved complete industry mastery.

This structural business analysis of network practices depicts well how the transformation of the television industry occurred. It surveys the processes by which, at the beginning of the decade, a multifaceted competitive industry offering a fare of single-sponsored live dramas became transformed, by decade's end, into an oligopolistic network-dominated industry offering telefilms sponsored by multiple advertisers.

Despite its impressive descriptive presentation of the acquisition of industry dominance by the television networks, there are several egregious lacunae in this work. Although Boddy discusses at great length the networks' concentration of influence within the industry, he does not consider how this process was influenced by external factors. Thus, he does not

investigate the changing nature of the medium's audience demographics, a significant element affecting both programming and sponsorship. Furthermore, despite his attention to television critics' motives, Boddy discusses neither program content nor any criterion for their aesthetic evaluation. After all, even if New York critics were in part motivated by parochial self-interest, their professional evaluations of television programming nonetheless require serious examination.

This inattention to specific program content and its critical evaluation contributes to Boddy's equivocal treatment of the pivotal notion of the 1950s as television's "Golden Age." On the one hand, in his introduction he refers to "the myth of television's Golden Age" (p. 5); on the other, he later refers to "the repudiation of the aesthetic tenets of the Golden Age" (p. 188), now understood as fact, not myth.

Some readers will also be disappointed that a study devoted to investigating the interaction of business interests and governmental regulation should virtually ignore the critical regulatory issue facing the industry at the opening of the decade: the future of CBS's commitment to, and the authorization by the Federal Communications Committee (FCC) of, a non-compatible color system and its eventual displacement by RCA's rival compatible color technology. Similarly, only glancing reference is made to experimentation with subscription television early in the decade and the FCC's eventual decision against this form of television programming and financing, a decision that profoundly affected the business dynamics of the industry.

Despite these shortcomings, the study is a welcome reevaluation of significant forces operating during the formative era of the American television industry.

JOSEPH H. UDELSON
Tennessee State University

JOHN J. BROESAMLE. *Reform and Reaction in Twentieth-Century American Politics*. (Contributions in American History, number 137.) New York: Greenwood. 1990. Pp. xvi, 481. \$49.95.

John J. Broesamle is a liberal, and more power to him! His is the story of victories and defeats. The Progressives were victorious for a while, then broke apart. The New Deal used the crisis of depression to make advances, then got diverted. The Kennedy-Johnson era expanded liberalism and was succeeded by conservatism and a growing confusion of direction among reformers. Mostly, the party of reform has been the Democratic. It has been the party of hope, the party that wanted to use government to extend rights to the deprived. There have been many directions and goals within liberalism, but "the one overriding thrust . . . has led it in the direction of the welfare state." This central focus has been weakened by "cultural radicals" who emphasize such issues as

sexual liberation and drugs. It has also recently been weakened by a "prissy" and "bloodless" neoliberalism that has lost touch with fundamental economic conflict. After each reform era, conservatism and reaction have taken over: "materialism," "self-centeredness," "fundamentalism" in religion, "flag waving," and, of course, probusiness and antilabor opposition to further extension of the welfare state.

There are at this moment plenty of issues for a revived liberalism, and Broesamle gives liberals some rules to follow: do not try to do everything at once; have specific goals; radicals may be necessary to formulate issues, but do not get distracted into a cultural radicalism that might offend allies in the middle or working classes; strong ideologies may be necessary to rally support, but ideologists cannot administer programs.

There is nothing new here. Most of Broesamle's references are to the major works of major historians, who are frequently quoted. Sometimes the references are to contradictory interpretations. Did the New Deal go in a totally different direction from Progressivism, or was it built on the formulations of the earlier reformers? Moreover, it is hard to understand, in a view of liberalism that puts the welfare state at the center, how the settlement house movement can go unmentioned.

Broesamle is more original in discussing the dynamics of reaction. That war blunts reform will not come as news, but Broesamle has some interesting comments on how the process works. He regards foreign policy generally as an escape hatch for presidents facing intractable domestic complexities. Reaction occurs as people become cynical about government generally, as intellectuals and the young abandon reform, and as the idealism of reformers comes up against actually administering oversold programs. The beneficiaries of welfare become less attractive and more threatening to the reformers. Thus, northern white liberals could support the integration of southern schools but not busing across school district lines in the North.

Whether the book is original or not, after a period when radicals called liberals nothing but disguised conservatives and conservatives called liberals nothing but disguised radicals, it is good to have the liberal (or progressive) tradition supported once again. After all, representational painting and tonal music are also again in fashion.

DANIEL LEVINE
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JOHN A. SALMOND. *The Conscience of a Lawyer: Clifford J. Durr and American Civil Liberties, 1899-1975*. Tuscaloosa: University of Alabama Press. 1990. Pp. xi, 265. \$34.95.

Few American lawyers have led more interesting or controversial lives than Clifford J. Durr. He served

the New Deal as an attorney for the Reconstruction Finance Corporation and fought for the public interest against self-serving broadcasters as a member of the Federal Communications Commission (FCC). Durr defended victims of McCarthyism and became a casualty of the politics of anticommunism. He was even one of those rare white southerners who openly supported the civil rights revolution. Although a failure by the monetary standards too often used to measure legal careers today, Durr accomplished far more than most of his nominally more successful contemporaries.

John A. Salmond has written a fascinating biography of this fascinating man. He skillfully explicates the evolution of a young attorney from a traditional southern background into a liberal reformer. Durr went to Washington, D.C., in 1933 only because he needed a job, but the New Deal, his brother-in-law (Hugo Black), and his activist wife (the former Virginia Foster) helped to reshape his thinking. By 1939, Salmond explains, "his political position was very different from what it had been in 1933" (p. 59). Salmond is equally good at explaining how Durr became an "implacable opponent, not only of [the House Committee on Un-American Activities (HUAC)], but of all institutions and individuals who tried to circumscribe the minds of men and women in the name of loyalty" (p. 98). Salmond characterizes Durr's 1942 investigation of Goodwin Watson, an FCC employee falsely accused by HUAC of disloyalty, as his subject's "personal 'Road to Damascus'" (p. 98). Perhaps the most insightful portions of the book are those dealing with Durr's racial views. Salmond makes it clear that this once-racist son of Alabama involved himself with the civil rights movement because he viewed it as "part of the continuing battle to prevent the further erosion of American civil liberties" (p. 171).

Although packed with rich insights into Durr's thought and motivations, the book suffers from a strange omission. It never mentions the death in 1951 of Durr's wife's sister, Josephine, and thus never offers an explanation for the fact that the Durrs apparently remained quite close to Justice Black even after his remarriage. Salmond also displays some reluctance to deal with the more technical aspects of Durr's legal practice, and, in discussing important appellate cases on which his subject worked, he fails to cite the published opinions of the courts that decided them. Although lawyers may fault Salmond for this omission, it is at most a minor flaw in a book that is really about the political odyssey of Clifford Durr rather than about his practice of the law and that is obviously intended primarily for lay readers rather than for members of the legal profession. More disturbing are the sometimes mangled syntax and occasional grammatical errors that mar this biography. At the very least, the mistakes suggest that the University of Alabama Press ought to hire better copy editors. It is a shame for readers to be distracted by

small and easily avoidable errors from the substance of a moving and informative biography of a truly remarkable American lawyer.

MICHAEL R. BELKNAP

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PAUL KENS. *Judicial Power and Reform Politics: The Anatomy of Lochner v. New York*. Lawrence: University Press of Kansas. 1990. Pp. 232. \$29.95.

The growing power of private business and the passage of the Reconstruction amendments, especially the Fourteenth Amendment, in the post-Civil War era set the stage for new departures in U.S. constitutional history. One key development was the Supreme Court's rethinking of the role of state governments in the use of the so-called police power and with it the emergence of what has become known as judicial activism—in that day an assertion of conservative values against public attempts to harness private greed and excess.

Paul Kens's engaging study addresses these larger issues in the context of the case of *Lochner v. New York* (1905), which, though hardly regarded as a quarrel that shaped the constitution, nonetheless symbolized the ideals of the Gilded Age at a time when those ideals were under severe attack. It also provided the basis for Oliver Wendell Holmes's famous dissent in which he asserted that "the Fourteenth Amendment does not enact Mr. Herbert Spencer's *Social Statics*." Approximately half of the book traces the background of the *Lochner* case, the nature of the baking industry in the United States in the late nineteenth century, the long struggle for maximum hours legislation in several states, specific political conditions in New York that made possible the passage of the Bakeshop Act of 1895, and the prevailing notions of laissez-faire economics and Social Darwinism that were generating spirited responses from Bellamyites, socialists, and moderate reformers. These chapters are informative and well paced; they outline interesting and in some instances little-known details. The remainder of the book deals with the case itself—its movement from the New York courts to the U.S. Supreme Court, where the maximum hours law was struck down by a five to four decision—and the meaning of the case through the first third of the twentieth century.

For Kens, *Lochner v. New York* had greater and longer-term significance than most students have maintained. The 1905 opinion, written by Justice Rufus W. Peckham, embraced the views of the late Justice Stephen Field who had been much influenced by Thomas M. Cooley's *Treatise on the Constitutional Limitations* (1868) as well as by classical economics and Social Darwinism. And, in joining substantive due process with the extraconstitutional doctrine of liberty of contract, Peckham's opinion evoked bitter criticism by judicial liberals and reform politicians

who argued that the federal courts would—and did—wield veto power over state regulatory legislation. True, admits Kens, many state laws did pass muster if they could be shown to protect public morals, peace and order, and health and safety, but even these were standards that had been established by the restrictive thinking of late nineteenth-century jurists. It was not until the case of *West Coast Hotel v. Parrish* (1937) that the *Lochner* era came to an end as new, more liberal standards were applied to regulatory legislation.

Although some scholars may not be totally convinced by Kens's insistence on the lasting effect of *Lochner v. New York*, this book provides a very good review of a series of important Supreme Court decisions against a background of profound economic, social, and political change. It is highly recommended to students of American history and government.

ROBERT F. WESSER
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CHARLES W. EAGLES. *Democracy Delayed: Congressional Reapportionment and Urban-Rural Conflict in the 1920s*. Athens: University of Georgia Press. 1990. Pp. xiv, 173. \$25.00.

The refusal of Congress to reapportion the House of Representatives between 1920 and 1929 was a unique occurrence in the nation's history. Moreover, historians have largely ignored the matter. This is unfortunate because the failure to reapportion not only increased rural overrepresentation in the House and the electoral college but also said much about the motives and procedures of senators and representatives in Congress during the 1920s.

Charles W. Eagles attempts, in this volume, to bring all of this to his readers' attention. The results of his study deserve notice. This is in part because of his blow-by-blow account of the reapportionment battles of the 1920s and in part because of his effort to use the reapportionment issue to test the influential thesis that conflict between rural and urban areas was an overwhelming force in American life during the period.

In paving the way for this test, Eagles provides a useful historiographical survey of the urban-rural conflict thesis and even of its interpretive competitors. Among the latter are the venerable concepts that American life during the 1920s was largely influenced by the results of World War I and that the decade was a battleground dominated by conservative reactions against Progressive impulses. Eagles also mentions later theses, for example, those on the encounters between Victorian and modern, local and national, and producer and consumer cultures. He leaves these topics for others to test.

Eagles elected to test the durable urban-rural conflict thesis by analyzing the eight roll call votes in Congress during the 1920s on the reapportionment

issue. His exhaustive analysis of these votes, broken down by census definitions both of rural and urban districts and of metropolitan and "most-rural" districts, yields illuminating results. Just as the issues debated in Congress and the press in connection with reapportionment included more than rural-urban ones, such as representation of aliens and blacks, the roll call vote analysis indicates that members of Congress were motivated in voting by factors other than rural-urban or some partisan concerns. These were state and individual congressional interests that were displayed in the often striking differences in votes between those states and their representatives that would gain or lose seats in the House. Eagles's careful analysis is a test on only one issue, but on that it seems that electoral self-interest instead of rural-urban or partisan concerns ruled the roost during the 1920s. Moreover, the resolution of the reapportionment issue in 1929, applied after the 1930 census, would lead to routine decennial House reapportionment afterward.

Eagles's work is an excellent exploration of a small but significant occurrence that tests a well-known historical theory. Historians should not only take note of the results of Eagles's scholarship but also his injunction that they more promptly test high-flown hypotheses with solid data instead of being so often satisfied that decades-long reiteration of a thesis makes it true.

DONALD R. MCCOY
University of Kansas

MARSHALL HYATT. *Franz Boas, Social Activist: The Dynamics of Ethnicity*. (Contributions to the Study of Anthropology, number 6.) New York: Greenwood. 1990. Pp. xii, 174. \$39.95.

When he died in 1942, German-born Franz Boas had been the most influential anthropologist in the United States for nearly four decades. Boas worked in every area of anthropology, including archaeology, linguistics, physical anthropology, and cultural anthropology. He recorded Indian languages, measured the rates of growth in school children, analyzed art styles, and traced folklore motifs. Behind the breadth of interests and methodological rigor for which he was famous lay a vision of how science could be used to better human society. Boas challenged the evolutionary model that placed contemporary European civilization at the top of the ladder of social evolution. He demonstrated that there is no necessary connection among race, language, and culture. He showed the plasticity of the human form and the effects of the environment on both mind and body.

Marshall Hyatt's interest is in the social activism that ran parallel to Boas's prodigious professional career. Boas's political activities ranged from defending individuals whom he felt were mistreated by institutions to defending Indian ceremonies, de-

nouncing spying by anthropologists in Central America, advocating American neutrality in the early years of World War I and prior to World War II, arguing against immigration restriction, and giving long-standing support for blacks in their struggle for equality. Hyatt suggests that many of the arguments used by the civil rights movement came from Boas, including the effect on blacks of a hostile social environment, the need for black pride, and the equipotential of the races. Boas was an early advocate of a black studies curriculum at Columbia. He was present at meetings that led to the founding of the National Association for the Advancement of Colored People in 1909. He gave a memorable commencement address at Atlanta University in 1906 about which W. E. B. DuBois wrote: "Franz Boas came to Atlanta University where I was teaching history in 1906 and said to a graduating class: You need not be ashamed of your African past; and then he recounted the history of black kingdoms south of the Sahara for a thousand years. I was too astonished to speak. All of this I had never heard and I came then and afterwards to realize how the silence and neglect of science can let truth utterly disappear or even be unconsciously distorted" (p. 99).

Hyatt finds three sources for Boas's political activism: the liberal ideas of the German revolution of 1848 that surrounded him as a child, his commitment to scientific accuracy and purity, and his own experience of anti-Semitism. He suggests that much of Boas's support for blacks was motivated by Boas's desire to root out prejudice of all kinds, including anti-Semitism, but that he used equality for blacks as his platform because he did not want to appear subjectively involved. Not until World War II did Boas begin to attack anti-Semitism directly.

Some of this book covers ground that will already be familiar to historians of anthropology. Yet it is useful to have a survey of Boas's career that emphasizes his political concerns and courageous public stands, and Hyatt's arguments are convincing, particularly his linking of Boas to the civil rights movement. What remains with the reader is Hyatt's final poignant description of Boas as "a humane man" who "was tormented by humankind's imperfections, and who desired nothing more than to help overcome them" (p. 157).

JOAN MARK

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PAULA F. PFEFFER. *A. Philip Randolph, Pioneer of the Civil Rights Movement*. Baton Rouge: Louisiana State University Press. 1990. Pp. xiv, 336. \$29.95.

Paula F. Pfeffer has produced a useful, if limited, study of the civil rights activity of A. Philip Randolph. Pfeffer argues that Randolph was one of the new breed of twentieth-century black leaders who orga-

nized the northern urban masses and broke with "clientage" politics. But, because he was the only major civil rights leader to emerge out of the labor movement, his politics were informed by a broader vision of the economic needs of African Americans than other leaders.

After summarizing Randolph's early career as a socialist militant during World War I and organizer of the Brotherhood of Sleeping Car Porters during the 1920s and 1930s, Pfeffer analyzes the ideologies and strategies that provided the blueprint for the civil rights movement of the late 1950s and 1960s.

The heart of the book addresses Randolph's many activities during and after World War II: he threatened a march on Washington, which resulted in President Franklin D. Roosevelt's issuance of an executive order banning discrimination in defense industries; planned a campaign of civil disobedience to end Jim Crow in the armed forces; opposed inequalities in the labor movement; organized the March on Washington in 1963; and contested "Black Power" militants in the late 1960s.

Although nonviolent civil disobedience has many parents, Pfeffer makes a good case for Randolph's role in spreading the tactic that Martin Luther King, Jr., made famous. She makes it clear, however, that Randolph never made a fetish of any tactic.

When Pfeffer examines the civil rights movement after the Supreme Court's desegregation decision of 1954, the book falters. There is a broad secondary literature here, and Pfeffer's research excludes much of the scholarship of the last fifteen years—David Garrow, Taylor Branch, and Clayborne Carson, to name a few. The chapters on the 1960s offer little, even on Randolph's role in the March on Washington, which he headed. At the same time, her discussion of the Negro American Labor Council (NALC), which Randolph founded in 1959 to combat racial discrimination in the American Federation of Labor and Congress of Industrial Organizations (AFL-CIO), suffers from the limits of the printed sources. It is surprising that Pfeffer does not mention any attempt to interview any of Randolph's associates in the NALC, such as Cleveland Robinson, or use the records of the AFL-CIO and the National Association for the Advancement of Colored People on this important subject often slighted in standard studies of the civil rights movement.

Discussion of the NALC might have been an avenue to explore Pfeffer's conclusion that Randolph's unique contribution was providing "a fusion of the economic and civil rights perspectives" (p. 303). If the civil rights movement had followed Randolph's perspective, she argues, "we would not today be lamenting the unfinished revolution" (p. 305). But the book's formal categories do not address her conclusion. Pfeffer adopts the contending dualisms popular in the 1970s—*interracialism and nationalism, masses and elites, and white paternalism and black assertion*. Nowhere is the relation of class and civil rights

systematically addressed. We receive hollow generalizations: "After Randolph's rise to prominence [during World War II], freedom from oppression would no longer be sufficient; from then on, blacks would demand their proportionate share of the nation's economic wealth" (p. 300).

Pfeffer provides a useful summary of Randolph's official positions and activities. Unfortunately, the book does not illuminate the important issues he addressed.

JUDITH STEIN
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SHELDON AVERY. *Up from Washington: William Pickens and the Negro Struggle for Equality, 1900-1954*. Newark: University of Delaware Press. 1989. Pp. 245. \$36.50.

William Pickens was a secondary, though significant, figure in the civil rights struggles of the earlier twentieth century. Sheldon Avery's work is a straight-ahead scholarly biography beginning with a very brief account of Pickens growing up in the 1880s and 1890s (drawn from Pickens's own autobiographical writings), one of ten children of a black Arkansas tenant farmer. Avery then follows the career of Pickens as a student at Talladega College and Yale University, his return to Talladega for a ten-year stint (until 1915) as an instructor, his increasingly active support of the newly formed NAACP (support that likely led to his dismissal from Talladega), and eventually his appointment to the staff of the NAACP as associate field secretary in 1920.

Unsurprisingly, well over half of Avery's text centers on Pickens's career as a staffer with the NAACP from 1920 to 1942. Pickens played an important role in efforts to increase the membership of local chapters around the country in the 1920s and the depression years. Avery also describes in considerable detail the role of Pickens in some of the most famous of the NAACP battles, including the rejection of Judge John J. Parker for a seat on the U.S. Supreme Court and the efforts to save the Scottsboro Boys. Avery, however, makes clear that Pickens was not an organizational "insider" and that he rarely played a central role in association decision making. Pickens worked well with James Weldon Johnson, as did most people, but he often came in conflict with W. E. B. Du Bois, editor of *The Crisis*, and increasingly found himself at odds with Walter White, Johnson's assistant and later his successor as NAACP secretary. Pickens did not give way easily once he was convinced of his views. That characteristic was seen clearly in his support for Marcus Garvey's Universal Negro Improvement Association (UNIA), given despite the opposition of his NAACP coworkers and much, if not most, of the black middle class. It is indicative of the man, however, that once he broke with Garvey (after Garvey endorsed the Ku Klux Klan), Pickens, in Avery's

words, "joined forces with the UNIA president's most outspoken critics" (p. 69). Throughout his career Pickens often seemed to take pleasure in being the odd man out. He would not adhere to an organizational line, whether that organization was Talladega College, the Garvey movement, or the NAACP.

Avery's account of the career of Pickens does add some interesting pieces of information, but most of what is in this biography has already been well established in the literature that has appeared over the past twenty-five years. In fact the work reviewed here is essentially the Ph.D. dissertation that Avery completed in 1970. The long delay in publication is a shame, for what might have been relatively fresh in the early 1970s now seems routine if not dated. Avery could have added breadth and depth to his published study by employing some of the analytical themes used in recent social historical studies and in works dealing with both individuals and organizations of the post-World War II civil rights movement. For whatever reasons, however, Avery stayed with his work of two decades ago and resisted either exploring the psychology of his central character or illuminating the civil rights movement of the earlier twentieth century by delving deeply into the broader social history of African Americans.

EUGENE LEVY
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RICHARD M. FRIED. *Nightmare in Red: The McCarthy Era in Perspective*. New York: Oxford University Press. 1990. Pp. ix, 243. \$22.95.

Those of us who teach recent American history have long needed an accessible text that offers our students a survey of the anticommunist furor of the late 1940s and 1950s. Richard M. Fried's new book fulfills that need—sort of. Because of the still controversial nature of what we, for lack of a better term, call McCarthyism, as well as its inherent conceptual complexity, Fried had no easy task.

His interpretation rests on the by-now standard view that McCarthyism encompassed more than McCarthy and grew out of a long-established antiradical tradition. He notes, correctly, that "much of the machinery and discourse of 'McCarthyism' was in place before the Cold War or McCarthy's advent" (p. 58). He also extends his engagingly written narrative beyond 1954 to show that the censure of McCarthy did not end the story.

Fried tries to be both comprehensive and balanced. He covers the famous cases from Alger Hiss and the Hollywood Ten to J. Robert Oppenheimer and the Rosenbergs. He also pays attention to the lesser-known victims and gives (as well as anyone can within two hundred pages) a good sense of the scope of the purges. In assessing the impact of McCarthyism, he ranges just as widely and shows how it produced "a significant slowing of the momentum of change in a

number of areas of American life" (p. 164). None of this is new; but it is new to today's students, and the breadth of Fried's inquiry is welcome.

Fried's search for balance is more debatable. As if he were writing in the 1950s when people shrank from taking sides on controversial issues, Fried offers his conclusions in an "on the one hand, on the other" format. Such a presentation, though superficially objective, does not always accord with good scholarship. Though many, if not most, of his conclusions are unobjectionable, others use straw men or rely on inadequate research as does Fried's intimation that Owen Lattimore's political and editorial "maneuvers" and "foolish utterances" had somehow contributed to his ordeal (p. 127).

Understandably, given the scope of his project, Fried depends on the scholarship of others, but not always with good results. There are, for example, factual errors—some trivial and some, such as his assertion that Reed College protected rather than fired a Fifth Amendment witness, not so trivial. More serious, however, is his failure to incorporate such major findings as those of Kenneth O'Reilly and Athan Theoharis on the FBI into his overall analysis. Fried seems not to recognize that the Freedom of Information Act and the scholarship of the 1970s and 1980s created new paradigms that require, for example, much more attention to J. Edgar Hoover's bureaucratic maneuvers than to McCarthy's media high jinks.

For some reason, Fried seems caught in an intellectual time warp, unable in many respects to extricate himself from the problematic of the McCarthy period itself. He asks no new questions and, thus, ends up with a victim-centered and anecdotal account that does little to advance our understanding of the complicated yet crucial interplay between the broad anticommunist consensus and the partisan pressures that precipitated the purges he describes. Nonetheless, the book is useful, largely because there is nothing else. When it appears in paperback, I will probably assign it, albeit with hesitation.

ELLEN W. SCHRECKER
Yeshiva University

A. A. FURSENKO. *Prezidenty i politika S.Sh.A., 70-e gody* [U.S. Presidents and Politics in the 1970s]. Leningrad: Nauka. 1989. Pp. 289. 2 r. 10 k.

A. A. Fursenko, a senior scholar in the Leningrad Division of the Institute of History of the Soviet Academy of Sciences and a Corresponding Member of the academy, is one of the most competent Soviet specialists on U.S. history in the twentieth century. His first visit to the United States was late in the 1950s, when he served as a guide to the first Soviet exhibit in New York, and he has been here many times since. His research has concentrated on the oil industry in the late nineteenth century. In fact, his

book on the Rockefeller family might have been a commercial success in the Soviet Union if Nelson Rockefeller had won Republican nomination for the presidency and had been elected president.

This volume, which follows one that Fursenko published on the American political-social crisis in the 1980s, suggests that Fursenko has decided to concentrate on interpreting contemporary U.S. history for the many urban Soviet citizens interested in American life. It concentrates entirely on Washington and the scandals and eruptions of the 1970s, except for one chapter on "the black problem."

Its sources include the memoirs of the leading political figures, from Nixon and Kissinger to Halde- man and Liddy. Fursenko also makes heavy use of books by Seymour Hirsch, Robert Woodward and Carl Bernstein, and Dan Rather, and reports of congressional committees. The *New York Times*, the *Washington Post*, and *Newsweek* are additional sources. Curiously, he does not refer to columns of observers such as David Broder or James Reston or newspapers published beyond Washington and New York. His concentration on the presidency and presidential politics leads him to neglect literature and the arts, education, and the economy, all of which affected his subject. In short, the volume reflects its sources and is as rich or as shallow as the American reporting that serves as his base.

Fursenko devotes almost a fifth of the volume to "the black problem." Seventy percent of these pages concentrate on the Jonestown tragedy in Guiana in November 1978, with Mark Lane the major source.

The author's conclusions resemble closely those of many American commentators on U.S. politics: Americans supported Reagan and chauvinism in 1980 because the sunbelt states grew in population and political power, transnational corporations acquired great influence, both political parties broke down, and the traditional ruling elites were bankrupt.

Fursenko obviously understands American politics at the visible Washington level, and he provides Soviet readers a summary of eastern press views. His volume represents considerable advance in Soviet understanding of the United States over such Soviet reporting and analysis because he has lived extensively in this country and has used American press sources. It replaces, however, all Soviet clichés with new American ones, another indication of increased American influence in the Soviet Union.

ROBERT F. BYRNES
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ALLAN KENT POWELL. *Splinters of a Nation: German Prisoners of War in Utah*. (Utah Centennial Series, number 6.) Salt Lake City: University of Utah Press. 1989. Pp. xv, 325. \$25.00.

During World War II the federal government transported approximately 371,000 German prisoners to camps in the United States. Of this number, approximately 8,000 Germans together with 7,000 Italians and 8,000 Japanese internees from the West Coast came to Utah. Some prisoners of war (POWs) in Utah were held at work camps in outlying areas, but the military kept most at installations in northern Utah's urbanized Wasatch Front.

Although Allan Kent Powell focuses on the German prisoners captured during World War II, he includes chapters on other topics as well. An opening chapter considers the experience of 517 German sailors from merchant ships interned with local aliens and conscientious objectors at Fort Douglas during World War I. Other chapters discuss the lives of Utahns captured by the Germans during World War II and of Italian POWs held in Utah.

Although focusing principally on the German prisoners from World War II, Powell writes separate chapters on different topics related to their imprisonment. In these chapters he considers their capture and shipment to the United States; the location and development of the POW camps in Utah; conditions in the prisons; continued POW loyalty to Germany and resistance to incarceration; attempted escape; employment; leisure-time activities; relations between prisoners and the local citizenry; and repatriation.

In addition, Powell devotes an entire chapter to the major tragedy in an otherwise exemplary experience. In this incident at a camp near Salina in July 1946, a mentally deranged guard, wielding a machine gun, slaughtered nine prisoners and wounded nineteen others. In typical cover-up fashion, American officials compounded this horror by failing to notify the prisoners' families of the deaths.

Powell underpins his writing with solid research. He relies on manuscript records, published reports and reminiscences, and personal interviews with a wide range of participants. Powell, who speaks fluent German, traveled to Germany to interview former POWs whom he located through the good offices of various governmental and private sources.

With the notable exception of the Salina massacre, the POW experience was perhaps as tolerable as one could expect under wartime conditions. American officials generally observed the Geneva Convention of 1929. Prisoners reported that they were well fed and well treated. Enlisted men were hired out to work in agriculture and nonmunitions activities. They were paid for this work at the same rate as an American private soldier. Some soldiers tried to escape. Surprisingly few succeeded, and those who did were almost invariably recaptured within a few hours. In spite of regulations to the contrary, the POWs initiated liaisons with Utah women and made friends with local people.

In assessing the value of Powell's work, it is important to note that although Powell focuses principally on conditions in Utah, his book offers a microcosm of

the POW experience throughout the United States. His conclusions that prisoners were generally well treated seem to be supported by the data collected. Prison life is never idyllic, but Powell's case study suggests that, with the exception of aberrations such as the Salina massacre, the German prisoners in the United States probably fared better than any others captured during the war.

THOMAS G. ALEXANDER
Brigham Young University

ROBERT GORDON KAUFMAN. *Arms Control during the Pre-Nuclear Era: The United States and Naval Limitation between the Two World Wars*. New York: Columbia University Press. 1990. Pp. viii, 289. \$40.00.

Robert Gordon Kaufman skillfully uses a historical perspective to illuminate his primary concern: the reappraisal of both the theory and practice of arms control and the lessons for current arms control negotiators. He concentrates on the three naval limitation treaties negotiated between 1921 and 1936 and, to a much lesser extent, on the Anglo-German Naval Treaty of 1936. In assessing these treaties, he focuses on United States thinking and policy, most especially on the challenges faced by the United States Navy in the Pacific from an increasingly expansionist Japan from 1921 to 1941.

Kaufman is generally critical of the arms limitation treaties. He argues that although it made sense to experiment with naval limitation from 1922 to 1930, steadily worsening conditions after 1930 transformed continuing efforts into dangerous folly. Treaties negotiated in the 1930s were based on fallacious assumptions about the causes and consequences of competitive naval armament, about the potential impact of new technologies on naval thinking and strategy, about the dynamics of Japanese politics, and about the intentions and capabilities of Japanese imperialism.

Five major lessons emerge: that unforeseen events can destroy the assumptions underlying naval arms limitation and thereby cause the treaties to fail; that political détente is the primary prerequisite to arms control agreements; that arms control treaties can seriously impede the meshing of foreign policy commitments with military capabilities and strategy; that democracies encounter major disadvantages in negotiating with more closed societies; and that, contrary to pacifist assumptions, a vigorous military building program acts as an important bargaining leverage and thereby promotes arms control agreements.

Kaufman has written a well-organized, lucid, and intelligent analysis of an exceedingly complex subject. He asks good questions, uses an impressive array of secondary works and primary sources, weighs his evidence carefully, and offers thought-provoking conclusions. Especially useful are his perspectives on the Washington Conference of 1921–22, secret Jap-

anese violations of the treaties, and the debates within the United States Navy over doctrine and strategy. As one who has long worked on the arms control conferences of 1927, 1930, and 1932, I am impressed with the efficiency with which he has compressed his findings into only 201 pages of text. The author further succeeds in his stated objective of offering a useful reappraisal of the theory and practice of naval arms control between the two world wars, one that scholars and negotiators will do well to consult.

Historians looking for a diplomatic history of arms control will be disappointed. Kaufman does not attempt to place the treaties in the larger diplomatic context of the 1920s and 1930s or to evaluate them in terms of their interaction with the other major diplomatic issues of the time. Accepting an interpretation of American foreign policy as an isolationism of means, if not of ends, he is seemingly unwilling to evaluate the larger international mosaic into which arms control diplomacy needs to be understood. Two important, albeit unsuccessful, conferences of 1927 and 1932, for example, are neglected. Yet these two conferences are significant to an understanding of the complexities of arms control diplomacy.

Use of a broader range of primary sources, such as those of the British Foreign Office and the British Admiralty, would also have led to a greater appreciation of the nature, variety, and inherent difficulties of arms control generally and, more specifically, of the diplomatic disputes and strains that influenced arms control diplomacy between the United States and Great Britain throughout the 1920s and into the 1930s.

Yet one cannot cover all of the important aspects of this complex subject in 201 pages or even twice that number. Despite its omissions, this book offers scholars and negotiators an intelligent perspective on arms control thinking in the 1920s and its defects after 1930. Equally valuable is Kaufman's excellent synthesis and evaluation of the secondary literature on the doctrines, strategies, and combat effectiveness of the United States Navy in the Pacific from 1922 to 1941. It is a significant and thought-provoking addition to our understanding of an important subject.

DONALD J. LISIO
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RICHARD G. HEWLETT and JACK M. HOLL. *Atoms for Peace and War, 1953-1961: Eisenhower and the Atomic Energy Commission*. Foreword by RICHARD S. KIRKENDALL. Assisted by ROGER M. ANDERS. (California Studies in the History of Science.) Berkeley and Los Angeles: University of California Press. 1989. Pp. xxix, 696. \$60.00.

This book is a penetrating account of nuclear policy making in the 1950s. The third volume of the official history of the Atomic Energy Commission (AEC), it provides a comprehensive survey of the debates over

the development and testing of new weapons, the generation of nuclear power, and the sharing of atomic technology for peaceful ends. It serves as a splendid addition to the series and adds immeasurably to our understanding of both Dwight D. Eisenhower and the AEC.

Richard G. Hewlett and Jack M. Holl describe in detail the critical years in the formulation of American nuclear policy. The atomic bomb had helped to end World War II, and the hydrogen bomb was nearly ready, but a variety of issues dealing with strategy and domestic use still had to be resolved. This book addresses the complex questions that Eisenhower faced in the period stretching from the first top-secret briefing he received at the Augusta National Golf Course in Georgia to his farewell address eight years later.

Hewlett and Holl contribute ably to the emerging revisionist portrait of Eisenhower. Eisenhower dominated nuclear policy, they suggest. He was intensely interested in atomic energy, aware of its possibilities but afraid of its awesome potential. Although he was determined to see development proceed, he insisted that it not jeopardize the economy in the government he sought. Far from being a passive president, he was an activist who played a powerful part in determining what was done.

Although the authors, both former chief historians at the Department of Energy, focus on Eisenhower and the AEC, they deal successfully with the scientific community and the often irascible members of Congress at the same time. Vivid portraits of the protagonists—Chairman Lewis Strauss and all of the others in policy positions—make the book far more than narrow institutional history. Indeed, this account is official history at its best.

Hewlett and Holl are hardly apologists for the agency. They are critical of the ill-conceived attack on J. Robert Oppenheimer that resulted in the loss of his security clearance. They note the AEC's loss of credibility in withdrawing an invitation to an important international conference from Hermann J. Muller, a Nobel Prize-winning geneticist, out of pique at his suggestion that radiation might produce genetic damage. Throughout their account they note questionable decisions and point to the limitations of the agency's approach. Often they single out Strauss for judicious criticism and note the consequences of his firm but sometimes misguided convictions. Finally, they provide an analytical focus for understanding the problems of the AEC as they underscore the never fully resolved dilemma of "how to promote and support the Administration's pursuit of the peaceful atom while at the same time exercising responsible control over its development" (p. 239).

ALLAN M. WINKLER
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WALTER L. HIXSON. *George F. Kennan: Cold War Iconoclast*. (Contemporary American History Series.) New York: Columbia University Press. 1989. Pp. xiii. 381. \$32.00.

This is the third biography we have had of George Kennan in the space of two years. What accounts for this persistent attention to a foreign service officer whose official career, but for a short stint as ambassador to Yugoslavia, ended in 1951? Perhaps the subtitle to Walter L. Hixson's engaging study provides the key. For forty years Kennan, the father of containment, has reinforced our doubts about the subsequent conduct of many aspects of U.S. foreign policy. As Hixson's meticulously researched study shows, the roots of Kennan's iconoclasm are to be found in his personality and values, but perhaps only an outlook of such primordial skepticism about the ability of our political system to provide intelligent policy could have sustained him in his role. In any case Hixson's delineation provides us with yet another occasion to examine the appropriateness of the premises on which U.S. policy operated for forty years (and continues to operate). It is from this angle that I think Hixson's study can be most profitably reviewed.

Kennan's personality and career illuminate the dilemmas and susceptibilities that endanger the intelligent conduct of U.S. foreign policy. As Hixson makes clear, much of Kennan's cold war iconoclasm stemmed from his skepticism about the American people and hostility toward the U.S. political system. In part, this was a function, Hixson observes, of his nostalgia for the mythical traditional cultures of pre-World War I Russia and Germany and in part sheer elitism. What he witnessed of U.S. life and culture appalled him and deepened his doubts and prejudices about America's capacity to fill the role of world leadership that it so assertively claimed for itself. In this light, Kennan's criticisms might be held to be prejudiced and suspect—the product of an egocentric elitist. But we now know enough about the specious values in terms of which U.S. policy is all too often justified and the irrationalities of the political process by which it is decided to see that perhaps many of Kennan's criticisms are justified. It is in this regard that Hixson's study of Kennan has a broader significance than as just an account of the idiosyncrasies of one man.

Hixson concludes his insightful analysis in the following terms. Far from being the embodiment of a consistent foreign policy realist, Kennan let his early hostility toward Soviet communism skew his analysis of the Soviet Union. In the positions that Kennan subsequently adopted toward postwar Germany, nuclear weapons, and the cold war, Hixson concludes, he "further distanced himself from the realist tradition." Hixson writes, however, with what I consider to be a more profound assessment of Kennan's contri-

bution as a private citizen, that despite his contradictions and confusions, there was a certain logic to Kennan's transformation from theories of containment to proponent of neo-isolationism (or at least *détente*). "He was right to abandon containment, a strategy whose formulation rested on too great a fear of communism and an exaggerated perception of the potential for the spread of Soviet power" (p. 308). Hixson also concludes, correctly I believe, that Kennan's contempt for the U.S. political system, albeit rooted in an excessively elitist set of values, had a justifiable basis in the American leadership's penchant for putting domestic political and bureaucratic considerations ahead of sensible calculations of the national and ethical interests of the country and in the American people's provincialism and embrace of romantic myths about their motives, thereby undermining their ability to conduct effective diplomacy. In that sense Kennan's iconoclastic views may be seen as an antidote to the excesses and disasters into which the public's ignorance and the concoctions of the politicians, the military, and the Central Intelligence Agency, among others, have gotten us. In this respect, Hixson's insightful biography enables Kennan once again to provide the helpful service that is the positive function of the iconoclast.

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Oxford, Ohio

FREDERICK F. TRAVIS. *George Kennan and the American-Russian Relationship, 1865–1924*. Athens: Ohio University Press. 1990. Pp. xix, 433. \$39.95

Frederick F. Travis's well-documented, meticulous, and judicious study of George Kennan, America's first specialist on Russia, is a striking example of historiographic overkill. Much energy, thought, and publishing funds were required to produce this three hundred eighty-four-page book (plus an excellent thirty-eight-page bibliography including all of Kennan's writings). But does the significance of the topic warrant such an extensive effort? Kennan's work was pioneering: his lectures, articles, and famous book, *Siberia and the Exile System* (1891), helped to arouse widespread public interest in Russia; he was a persuasive defender of the rights of Russian opponents of tsarist oppression; and his activities and sympathy undoubtedly encouraged those members of the Russian revolutionary movement whom he knew. Yet these aspects of Kennan's career could have been adequately treated in two or three sharply focused articles. Historians need to concentrate major research and writing on subjects of unquestioned significance.

As Travis acknowledges, Kennan's writings and lectures had little influence on American foreign policy or on the course of Russian-American rela-

tions. Kennan had access to President Theodore Roosevelt and to Robert Lansing, Woodrow Wilson's secretary of state, but Kennan's views on Russia were largely ignored by Roosevelt and proved primarily confirmatory in Lansing's case. In the 1890s Kennan and his supporters failed in their effort to have the terms of an extradition treaty with Russia changed, and in the first decade of the twentieth century a group of prominent American Jews, rather than Kennan, led the successful fight to abrogate the Commercial Treaty of 1832 with Russia. Public outrage over pogroms in Russia, not reaction to Kennan's condemnation of the tsarist system, fueled the campaign for abrogation, which was designed to punish the Russian government for mistreatment of Jews. Like the Jackson-Vanik amendment of 1974, cancellation of the treaty had no impact on governmental policy in Russia and primarily damaged American foreign traders and investors.

Travis raises two interesting issues in his study. One is the persistently unsolved question of how to measure the impact of writings or speeches on public opinion. Did Kennan's castigation of the tsarist government's treatment of political exiles arouse his readers and listeners against official Russia? If so, did this affect U.S. policy toward the tsarist state? At one point, the author seems to answer these questions affirmatively by stating that "Kennan's conversion [from a pro-tsarist to an anti-tsarist position] . . . had far-reaching and long-lasting results on American-Russian relations, especially in the broader non-diplomatic sense" (p. 112). Nevertheless, the body of Travis's work suggests almost exactly the opposite. What public opinion is, how the work of publicists affects it, and the degree to which it influences policy remain difficult issues that neither the social scientist nor the historian has fully resolved, even in this era of instant polling.

A second challenging question is the responsibility of the journalist-observer to provide a complete and balanced picture of the society or event being described, an issue raised recently in S. J. Taylor, *Stalin's Apologist: Walter Duranty, the New York Times' Man in Moscow* (1990). As Travis makes clear, Kennan focused on one aspect of a complex system and espoused unwaveringly from 1886 until his death in 1924 the liberal solution to Russia's problems. This was commendable in terms of promoting human rights, but it left Kennan's audience only partly informed about the situation in Russia and largely unprepared for the later Bolshevik revolution. Significantly, Kennan did not know and made no effort to become acquainted with such early pioneers of academic Russian studies in the United States as Samuel Harper at the University of Chicago and Archibald Cary Coolidge at Harvard University.

This volume deals clearly and effectively with every aspect of Kennan's work on Russia. Unfortunately,

the importance of the topic does not match the quality of its treatment.

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MITCHELL K. HALL. *Because of Their Faith: CALCAV and Religious Opposition to the Vietnam War*. (Contemporary American History Series.) New York: Columbia University Press. 1990. Pp. x, 231. \$39.50.

This solid and interesting monograph traces the development of Clergy and Laymen Concerned about Vietnam (CALCAV), the largest explicitly religious group formed to oppose the Indochina war. After six months of local and ad hoc efforts under various names, CALCAV began full-scale operation in April 1966. Most of its leaders were Protestant and Jewish theological liberals, including John C. Bennett, Abraham Heschel, William Sloane Coffin, and Richard John Neuhaus; Martin Luther King, Jr., served as cochair. By 1972, CALCAV had a national network of locals and forty thousand largely middle-class members. They lobbied, picketed, rallied, produced radio and television spots, and published a magazine called *American Report*.

This study details the growing militancy of most members along with the disaffection of those disturbed by that militancy. Keeping its distance from the ideological Left, CALCAV never endorsed unilateral withdrawal from Indochina and cherished instead the (probably chimerical) hope of a negotiated peace. Yet the failure of polite protest to end the war moved some organization leaders and many of the rank and file to join in civil disobedience, support selective conscientious objection, and aid draft evaders abroad. They also increasingly believed that the war revealed basic flaws in American society. In particular, CALCAV activists attacked corporate power in general and focused attention especially on the war-related activities of International Telephone and Telegraph, General Electric, and Honeywell. Starting in 1970, several locals highlighted this broader agenda by using the name Clergy and Laymen Concerned (CALC).

As Hall shows, most CALCAV members, along with most of their fellow citizens, preferred to think of the war as a mistake rather than a symptom of deeper social flaws. Moreover, CALCAV's drift leftward inadvertently provoked some of its early leaders, including Neuhaus and Michael Novak, to move right. In the hyperbolic judgment of another disenchanting member, sociologist Peter Berger, the organization degenerated into a "Maoist chaplaincy" (p. 123). Despite such defections, CALC survived after the war as a "multi-issue social-justice organization" (p. 169).

Hall places CALCAV in the context of the wider

peace movement and developments within individual religious denominations. Ultimately the organization attracted some Roman Catholics and evangelicals, thus serving to enhance the cause of ecumenicalism. Hall writes fluently and has made extensive use of interviews, FBI files, and diverse manuscript collections. Then, too, he understands that actions by rank-and-file members of this (or any organization) cannot be inferred simply by reading statements issued from the national office.

Perhaps modesty prevented Hall from placing the story in a wider perspective. For example, he might have usefully reflected on the connections (or lack thereof) between theological premises and political positions and analyzed (rather than noted) the transitions by Neuhaus, Novak, and others from left liberalism to neoconservatism. What Hall chooses to do, however, he does well. This book is a valuable addition to the small but growing literature that treats the "sixties" as history rather than as hagiography or demonology.

LEO P. RIBUFFO
George Washington University

CANADA

PHILIP LAWSON. *The Imperial Challenge: Quebec and Britain in the Age of the American Revolution*. Buffalo: McGill-Queen's University Press. 1989. Pp. x, 192. \$32.95.

The separatist movement in the Province of Quebec depends on the existence of a political unit with a French-speaking majority, its own civil law code, and a Roman Catholic heritage. That province's existence is traceable to two British parliamentary bills: the 1774 Quebec Act and the Constitutional or Canada Act of 1791. Philip Lawson's well-researched book reconstructs the British political context from which the Quebec Act emerged. It is a complex tale of how decisions result from the interplay of rival government bodies, diverse characters, and conflicting priorities. As an account of administrative decision making, the book can be fascinating. This is not an introductory work for the uninitiated; Lawson sometimes refers to episodes, such as "the Walker affair," without explanation. His tale has a happy ending because temperate, pragmatic, and humane men concerned with the feelings of the newly conquered *Canadiens*, with securing the former French colony to the British empire, and with the welfare of commerce prevailed over anti-Catholic bigots. The result was an act that confirmed Roman Catholic freedom of worship, restored French law in matters relating to property and civil rights, sanctioned collection of the tithe, provided an oath for public offices acceptable to Roman Catholics, and permitted the governor and his council to pass laws. Religion is presented as the central issue in the debate, and the author suggests that discussion of Quebec's fate pushed Britain along

the road to Catholic emancipation. George Savile's toleration bill of 1778 is mentioned, but the connection is not demonstrated. The barriers to *Canadien* participation in government were religious ones, and legalization of the church tithe was necessary to support the Roman Catholic clergy. How religion was central to the preservation of French civil law is not apparent. The grant of legislative powers to the governor and council and the withdrawal of the promised assembly were said to be necessitated by the impossible choice between granting Roman Catholics full political rights and having a legislature controlled by the small Protestant minority. The *Canadien* majority had no experience of representative government. The time was ripe for toleration but not for enfranchisement.

The writer is a self-conscious revisionist, and parts of his narrative are novel. He shows that the basic outline of the Quebec Act was sketched out by the Rockingham government in 1766, but politics and a decision to change Quebec's civil government by parliamentary act rather than by order-in-council delayed implementation. Solicitor-General Alexander Wedderburn's report of 1773 on Quebec is described as "the very basis of the Quebec Act. . . . Historians really need look no further than this document for the immediate source and impetus behind the Quebec legislation" (pp. 120, 122). Historians usually emphasize the role of Governor-General Sir Guy Carleton, Lord Dorchester, who was a champion of the *Canadiens* and a persuasive witness in support of the bill. Lawson demonstrates what previous authors with less evidence have asserted: that the Quebec Act merely coincided with the "Intolerable Acts" and owed nothing to developing unrest in the older British North American colonies. The act was a specific solution to the problem of governing a former French colony with a large, established European population possessing its own institutions. British radicals, however, charged the bill with being an entrenchment of "popery" and despotism with an eye to extending them elsewhere in the British empire. The radicals' unfounded accusation was then appropriated by authors of the American Declaration of Independence, and, thus enshrined, it became a durable myth. Quebec nationalists are reluctant to acknowledge their debt to British administrators and legislators who aided French-Canadian survival; in 1989, Montreal's Dorchester Boulevard was renamed Boulevard René Lévesque after a recently deceased *Parti Québécois* premier heralded as the nation's regenerator. Alas poor Dorchester—only the historians will remember you.

PETER MOOGK
University of British Columbia

PETER WARD. *Courtship, Love, and Marriage in Nineteenth-Century English Canada*. Buffalo: McGill-Queen's University Press. 1990. Pp. x, 219. \$24.95.

Peter Ward's goal is to chart the "elaborate web of regulations which guide private conduct" from courtship through the wedding day in central and maritime English-speaking Canada during the long century from the 1780s to the eve of World War I. He argues that Edward Shorter, Lawrence Stone, and Peter Gay, by their emphasis on growing personal autonomy and the psychological forces that shape personality, have underestimated the continuing tension between the community interest in who should marry whom and couples' search for privacy in their intimacy. In particular, he discusses the religious, legal, demographic, and wealth-holding contexts within which courting was conducted. He discovers that Canadian marriage-age patterns conformed to European forms: grooms were generally older than their brides, and both parties were older at marriage as the century wore on, women being less likely to find marriage partners from the mid-century. Early on, men organized courting opportunities by sponsoring public occasions, women by invitations to private parties, although through the years bachelors discarded their roles as social convenors, while women continued to rely on domestic scrutiny to protect their match-making interests. Overall, community rituals governing marriage were weaker in English Canada than they had been in Europe but lingered longest in the rural areas of eastern Ontario and the Maritimes. Parental opinion held sway over marriage decisions for women longer in the century than for men, and was more persistent in families of wealth than among those of modest means.

The book is gracefully written. Ward has discovered a large number of courtship narratives, and presents them with an enthusiasm for the small detail alternately engaging and discomfiting to the reader. Despite his prefatory declarations about the importance of changing social constraints on courtship, Ward concludes that "when it came to love, things seem to have changed very little during the century, and they differed surprisingly little from one couple to the next" (p. 167), marriage offering "the only possible way to transform romantic love into lifelong intimacy" (p. 168). There is a blissful quality about the book, and an attendant inclination to evade the tough questions that emerge from the evidence.

Ward confuses responsibility with authority, arguing that the household reliance on female labor sustained a women's sphere, narrower but not necessarily subordinate to men's. He mistakes formal for substantive equality, citing as a sign of progress the frail new married women's property act that left control of property transfers and women's wages in their husbands' hands, and arguing that male domination lent support "to the solidarity of family life" (p. 49). He locates the pattern of older men taking younger brides in a "marriage market" framed by demography rather than differential authority, acknowledging only hints of female submission in a courtship process that was to end with a one-party

promise to "honour and obey." Throughout, love's power to transcend is celebrated, while the lovers' differential access to privilege is appraised quizzically and with caution. Readers familiar with the recent work of Wendy Mitchinson, Andr  e Levesque, Carolyn Strange, and Karen Dubinsky on women's experience of sexuality in this period will find in Ward a distinctly dissonant view.

JOY PARR
Queen's University
Kingston, Ontario

JOY PARR. *The Gender of Breadwinners: Women, Men, and Change in Two Industrial Towns, 1880–1950*. Buffalo: University of Toronto Press. 1990. Pp. xiii, 314. Cloth \$45.00, paper \$19.95.

As the title of this book suggests, Joy Parr's study of industrial change between 1880 and 1950 is informed by feminist historiography, but its theoretical and empirical base is much broader. Despite Parr's sensible insistence that "the character and precedence of class and gender identities are a matter of history, not universals but specificities" (p. 11), this volume is much more than a simple case study. Its findings will also interest students of technology, management techniques, and, especially, industrialization.

The work is based on a thorough examination of two southern Ontario communities, the "women's town" of Paris, a knit-goods manufacturing center whose mainly female work force had been largely recruited from the English Midlands, and the "men's town" of Hanover, a furniture manufacturing center, many of whose workmen had German roots. Parr builds her study on intensive research in the extensive records of Penmans Limited, a knitting company, and the Knechtel Furniture Company as well as local newspapers, government records, and interviews with over sixty former workers. By linking payroll records and municipal assessment files, she ascertains the employment history and household status in select years for a significant number of workers in both industries.

The book consists of separate accounts of each of the communities under study and an interwoven theoretical analysis. It can be read at two levels. As a case study it offers vivid pictures of daily life on the factory floor, at home, and in the community. We read in detail how knitters and woodworkers made stockings and underwear or furniture; we learn of the paternalism at Penmans, which included flexible working hours for mothers of young children, and of the Knechtel's unsuccessful experiment with scientific management; we discover how extended families in Paris shared child care and domestic duties and how men at Hanover could afford to keep their wives at home by increasing family resources through raising gardens, making and selling crafts, and working extra hours; we also observe the failure of a militant strike

at Penmans and the increased strength a strike brought to a union of furniture workers.

Despite its organization and content, the book is not really a comparative study, and Parr usually resists pressing gender comparisons too far. Yet her provocative suggestion that the protective tariff can be perceived as a gender issue is not convincing because the Canadian textile industry was traditionally handsomely protected by tariffs.

Nevertheless, the wealth of detail allows Parr to challenge or moderate several theories about gender and class. For example, she notes that sexual segregation in industry can vary from place to place, as in the case of certain jobs in the knitting industry, which in the East Midlands were considered men's work but in Ontario were the task of women. Similarly, she contends that theories of work that overlook working men's appreciation of craft, agricultural, and family traditions may be wanting.

Indeed, the main lesson to be learned from this study is Parr's insistence that gender, class, community, ethnicity, and religion (a point she does not really examine) "cannot be teased apart into separate analytical threads without causing the fabric of the social experience to disintegrate but must be analyzed as a web of interdependence, densely felt into a single whole" (p. 227). In showing the complexities of life and the pitfalls of one-dimensional analysis, Parr has made a challenging contribution to our understanding of working-class societies and their histories.

PATRICIA E. ROY
University of Victoria
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PATRICIA E. ROY. *A White Man's Province: British Columbia Politicians and Chinese and Japanese Immigrants, 1858-1914*. Vancouver: University of British Columbia Press. 1989. Pp. xvii, 327. \$37.95.

In this study Patricia E. Roy explores the interrelated factors of race, economics, and politics in British Columbia between 1858 and 1914. Roy, defining race by skin color and other unchangeable physical characteristics, examines how the attitudes of white British Columbians toward Chinese and Japanese immigrants changed from a level of tolerance to outright hostility. It is a classic example of the conflict between capital and labor with the added factor of race used by the politicians and media to maintain white culture.

Roy begins with the gold rush, the initial incentive for attracting white and Chinese immigrants to British Columbia. White miners from California brought prejudice against Asians with them, but the Chinese were not perceived as economic competitors. Roy argues that the real concern of white British Columbians were the Chinese customs of sojourning and living frugally, which resulted in the Chinese not

contributing their monetary share to the colonial revenue.

In two chapters covering the period from Confederation to the Great War, Roy explains how the Chinese and Japanese resided in segregated areas of settlements, where few whites had contact with them outside the competitive market, and how perception of them became hostile. Negative images of Asians were perpetuated in the media by reports on their immorality, housing conditions, and unsanitary lifestyles. Roy argues, however, that over time the real fear of white British Columbians was economic competition. Prompted by thousands of Chinese immigrants employed by the Canadian Pacific Railway and in the coal mines on Vancouver Island and the Fraser River salmon canneries at lower wages than white labor, white British Columbians pressed the provincial and federal governments to pass legislation restricting Asian immigration.

Roy explains the political process of checking Chinese and Japanese competition between 1886 and 1896. She examines the success of the federal government's Chinese Immigration Act, which designated a fifty-dollar head tax, assesses the attempts by the provincial politicians to introduce a "no Chinese" clause into the Coal Mines Regulation Act, and evaluates the campaign of the Fisherman's Association for restriction of Japanese fishermen, who held 20 percent of the fishing licenses issued in 1893.

From 1896 to 1902, provincial and federal politicians could not agree on the principle of restricting Asian immigration. Most provincial politicians were in agreement, but the government of Sir Wilfrid Laurier did not view Asians as a problem in eastern Canada. Provincial politicians pressed their concerns by proposing increases in the head tax and introducing the Natal Act, a language test. In response the federal government used a classic Canadian device to defer decisions and take the heat off politicians; it established a royal commission on Chinese and Japanese immigration in 1900. Despite the fact that Laurier acted on some of the recommendations, such as increasing the Chinese head tax to five hundred dollars, British Columbia still faced problems of checking economic competition because the legislature operated under five different premiers between 1898 and 1900. Compounding the political instability of the province was the contest between provincial legislation and the federal power of disallowance as well as imperial policy. The Anglo-Japanese Treaty of Commerce and Navigation curtailed overt discrimination against the Japanese.

Roy also examines the economic background of the province and the eventual violence that occurred in Vancouver. From 1903 to 1907 the provincial economy flourished, and, with a shortage of labor British Columbia was attractive to Asian immigrants. The politicians refused to let the Asian question disappear and made increasingly racial arguments against Asians. The racism manifested in the Vancouver rally

organized by the Asiatic Exclusion League and ended in a riot in Chinatown and the Japanese quarter of the city. A major consequence of the violence was an agreement limiting the entrance of Japanese contract labor in the mining, fishing, and lumbering industries.

In the final chapter, Roy summarizes the attitudes of white British Columbians toward Chinese and Japanese immigration and their home countries between 1908 and 1914. Although there were different perceptions of the two groups, there was complete agreement that British Columbians should continue to make "a white man's country."

One would expect a well-researched, detailed volume from Roy, who has been studying Canadian-Asian history for a good portion of her career. This is certainly the case with this book. It is not only a major contribution to the historical literature of British Columbia but also an excellent study on the topic that will be of comparative value to other scholars interested in Asian studies in California and Australia. The author has included forty-five pages of notes for researchers.

There are a number of minor irritations that are worth mentioning. Repetition could have been eliminated. I was surprised by a comment on page 188 about the abundance of archival photographs because a few of them would have been a welcome addition to the text. Also on page 188 there is a discrepancy in the same Japanese immigration statistic that is mentioned on page 186. Finally, the epilogue could have been left out because Roy is planning a sequel.

PAUL M. KOROSCIL
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LATIN AMERICA

JOSEPH L. LOVE and NILS JACOBSEN, editors. *Guiding the Invisible Hand: Economic Liberalism and the State in Latin American History*. New York: Praeger. 1988. Pp. x, 222. \$38.95.

The appearance of this study as well as several other new works indicates that the nineteenth century in Latin America is finally receiving the kind of scholarly analysis that it deserves. Alternatively branded by the followers of dependency theory as the period when the Latin American elites sold their countries down the river of free trade and by nationalists as the dark days of caudillos interested in little but self-enrichment, the nineteenth century and the decades following independence in particular have been condemned to virtual obscurity until the last few years.

In this collection of seven essays, editors Joseph Love and Nils Jacobsen bring fresh light to the complicated evolution of economic liberalism in Latin America during that troubled time. In part the volume reflects a shift away from studies of export economies to that of the internal economics of the

region. The result provides a more complex and satisfying explanation of the reasons why the region adopted free trade policies. It also indicates that historians can study how Latin Americans themselves understood the benefits of economic liberalism.

Each essay here has its own distinctive approach to the issue of the state's hand in commerce. Love, for example, compares the development of free trade ideology in Latin America with that in Romania. In so doing, he notes that *narodniki* populism based on a "socialist" institution already indigenous in Russia, the *mir*, had great influence on Eastern European thought but none in Latin America, which had similar "socialist" institutions. His article reminds historians of Latin America (and of Europe for that matter) how much can be learned by employing comparisons.

Frank Safford and Paul Gootenberg, writing on Colombia and Peru, respectively, lay to rest the notion that Latin American regimes adopted free trade very quickly and easily after independence. Instead, they focus our attention on how protectionists won the day consistently from the 1820s until the late 1840s despite the best efforts of the foreign merchant community and its allies.

Tulio Halperin Donghi compellingly presents the inner contradiction of liberalism in an Argentina supposedly "born liberal," because of a past more instinctively democratic and egalitarian than that of other "more feudal" Spanish-American republics (p. 101). Ironically, the state's refusal to intervene in land issues after 1861 made Argentina into a nation of renters whose insecurity would eventually doom Argentine liberalism. Steven Topik, in contrast, compares Mexico and Brazil on a macroeconomic level to determine their separate interpretations of liberal free trade economics. From his gathering of statistics, he concludes that, in the late nineteenth century, Brazil was decentralizing politically while intervening substantially in the economy, whereas Mexico was undergoing precisely the opposite transformation.

As historians delve even more deeply into the impact of free trade economics on specific regions, they might find, as did Jacobson in his article on southern Peru, that local interests supported free trade in order to survive against more developed industrial capacity in the other parts of these countries. Florencia Mallon's concluding essay argues that historians need to investigate the impact of trade decisions on both regions and sectors of society.

This book, then, contains a very interesting collection of essays for teachers and scholars alike. Regrettably, however, it includes neither a "guiding" introductory essay nor a map.

BARBARA A. TENENBAUM
Encyclopedia of Latin American History

PAUL E. HOFFMAN. *A New Andalusia and a Way to the Orient: The American Southeast during the Sixteenth Cen-*

ture. Baton Rouge: Louisiana State University Press. 1990. Pp. xiii, 353. \$42.50.

This study by Paul E. Hoffman is about legends and exploration in the sixteenth century. The first half of the book's title, *A New Andalucía*, is derived from Lucas Vázquez de Ayllón's description of the Atlantic Coast of what is now the southeastern United States, a region he had then never visited. The description created the Chicora legend, which stated that the Southeast was a new Andalucía and comparable to the Spanish Andalucía in agriculture, mineral wealth, and climate. The Chicora legend and the belief in the existence of a Northwest Passage across North America ("a way to the orient"), which started about the same time with Giovanni da Verrazzano, attracted a number of supporters who searched for them throughout the sixteenth century.

Hoffman recounts how the Spaniards after the discovery of America initially employed diplomacy to exclude non-Iberians from the Western Hemisphere. But that broke down as corsairs, lured by both real and imaginary wealth, began to infest the Caribbean. Although the Chicora legend lapsed into disrepute among the Spaniards after the failures of the expeditions of Ayllón and Hernando de Soto, Tristán de Luna's effort at settlement and conversion on the Mexican Gulf Coast of 1559–61 helped revive it. Also important in stimulating the imagination of French and English interlopers in New World riches was the publication of books on exploration and geography (by, among others, Peter Martyr, Gonzalo Fernández de Oviedo, Francisco López de Gómara, Garcilaso de la Vega, and the "Gentleman of Elvas").

In the 1560s, under Pedro Menéndez de Avilés, the Spaniards seized La Florida (Florida, Georgia, and the Gulf Coast) and ousted the French. Hoffman does not elaborate here as other studies have already told the story. The Spaniards discovered in this region only rebellious natives and sandy soil as the new Andalucía continued to elude them.

In the late sixteenth century, more publications containing new information (by Richard Hakluyt the Younger, Richard Eden, and Giovanni Battista Ramusio, among others) propelled English navigators into exploration and eventual settlement in the lands north of the Spanish dominions. By then the Spaniards discounted the Chicora legend and the possibility of a North American route to the Orient. They clung to peninsular Florida in order to protect their treasure fleets that sailed from Havana for Spain.

In tying together the initial voyages of discovery and the creation of legends, Spanish settlement efforts, diplomatic treaties, the publication of books on geography and exploration, the sharing of information by the English and the French, and the follow-up voyages, Hoffman tells a fascinating story. He has researched the Spanish archives and has used solid monographic studies. For English and French explo-

ration, he has relied on the secondary literature. His study is useful in explaining the interrelationships among the activities of several European nations on the Atlantic Coast in the century preceding the founding of Jamestown. Hoffman's book contains important information that should be incorporated into standard U.S. history texts.

GILBERT C. DIN,
EMERITUS
Fort Lewis College

B. W. HIGMAN. *Jamaica Surveyed: Plantation Maps and Plans of the Eighteenth and Nineteenth Centuries*. Kingston: Institute of Jamaica Publications. 1988. Pp. xv, 307. Cloth \$55.00, paper \$30.00.

VERONT M. SATCHELL. *From Plots to Plantations: Land Transactions in Jamaica, 1866–1900*. Mona, Jamaica: Institute of Social and Economic Research, University of the West Indies. 1990. Pp. xiii, 197. \$16.50.

Barry W. Higman's magnificent study marks a turning point in the historiography of Jamaica. It is a beautiful and an important book. Its reason for being is the rich Jamaica National Library collection of plantation maps, which provides information on more than twenty thousand properties. The collection is unique for the Caribbean region and perhaps for the whole world: a relatively untapped scholarly resource and an invaluable key to one of the purest plantation societies in world history. As Higman explains, "Although the large plantation typified the relations of production in the slave societies of Brazil and the United States, the plantation itself remained something of a myth, most slaves living outside its physical context. In Jamaica myth and reality converged" (p. 5).

Despite its size, however, the collection includes only about one thousand plans that depict the internal, spatial organization of the properties. This study is drawn from that portion of the collection, amplified by paintings, seals, blueprints, scale drawings, cartouches, aerial photographs, and much else, and by the detailed and informative texts that Higman has supplied.

There are ten chapters in which Higman discusses not only plantations, pens, plantation houses, and gardens, grounds, and mountains but also surveying and plan making, including a good deal on the surveyors themselves. The book overflows with maps and illustrations and provides a rich and illuminating vision of Jamaican land and the history of its uses.

There is no doubt that this book will prove itself an invaluable research tool for specialists in Caribbean history. Its value is much enhanced by Higman's insightful comments on the history of land use and on the changing fate of rural folk in a country so long and so powerfully committed to a large-scale agricultural destiny.

Veront Satchell's book was published two years

later than *Jamaica Surveyed* but can profitably be reviewed with it. Satchell is Higman's student and was one of his research assistants in the preparation of *Jamaica Surveyed*. Satchell's subject is land transaction, 1866–1900, and his object is “to identify the changing patterns of rural landownership in Jamaica between 1866 and 1900 through a statistical analysis of the land conveyance deeds recorded at the Island Record Office” (p. 1). Satchell divides those thirty-four years into four segments so that they may be compared with each other: 1866–69, 1870–79, the 1880s, and 1890–1900. During the first, the reintroduction of crown colony government following the Morant Bay rebellion slowed the resolution of a chaotic land situation; smallholders, many or most of them freed-persons or their children, were the most active dealers in land during this period. In the second, titles were “straightened out,” and squatting ended. Satchell demonstrates that increased leasehold by the government came at a time when “squatters were ejected from thousands of acres of land . . . which the Government now offered for lease” (p. 88). Although smallholder banana production began at this time, there was also a discernible reconcentration of land. A quotation from the surveyor-general in 1874 makes it clear that large-scale squatters—and there were many—were not always treated the way small-scale squatters were (p. 72). In the 1880s, banana growing began to change into a plantation enterprise, even as the sugar industry continued to decline. In the final period, 1890–1900, some measure of government concern for smallholders is evinced, but by then the government had little crown land left to bestow on the peasants. Satchell does not deal with the rather cruel irony of a policy of ejecting squatters, followed by the official discovery that there was no land for the landless. Large estates became a predominant feature of the Jamaican landscape yet again.

Together these works afford us a remarkable survey of a classic “sugar island,” where plantation and peasant have been warring for at least one hundred fifty years. As has been true everywhere else in the region, all of the weapons except slavery remained in the control of the large-scale estates. That Jamaican peasants, like Caribbean peasants elsewhere, fought a losing battle is much less remarkable than that they fought so well and so long, with little on their side but their love of the land.

These books are, on the whole, excellently prepared and executed. (There is a noticeable typographical breakdown on page 32 of Satchell's book; it is otherwise mostly well done.) Higman's volume, handsomely turned out, was printed in the United States and published by the Institute of Jamaica Publications. The authors deserve our thanks and merit our praise.

SIDNEY W. MINTZ
Johns Hopkins University

CHARLES A. HALE. *The Transformation of Liberalism in Late Nineteenth-Century Mexico*. Princeton: Princeton University Press. 1989. Pp. xi, 291. \$37.50.

The publication of this book brings to completion over two decades of research and writing by Charles A. Hale on the intellectual life of nineteenth-century Mexico. His first work, *Mexican Liberalism in the Age of Mora, 1821–1853* (1968), traced the origins of liberalism in Mexico from its European antecedents and the role of liberalism's chief advocate and architect, José María Luis Mora. In the current work, Hale narrates the story of liberalism in Mexico from 1867 to 1910. He analyzes the classic “atomistic” liberalism of the Constitution of 1857 and then shows how liberalism was transformed into an organic doctrine by the advocates of scientific politics between 1878 and 1903. The latter formulation of liberal politics was greatly influenced by Comtean positivism and the model of late nineteenth-century conservative republics in Spain and France. In his concluding chapter, Hale suggests that the heritage of liberalism continues into the twentieth century in the form of the “constitutionalism” of Venustiano Carranza in 1917 and the democratic Caesarism of today's PRI-dominated Mexico.

Hale argues that liberalism triumphed in 1867 with the victory of Benito Juárez over Emperor Maximilian and the native Conservative party. Between 1867 and 1910 liberalism was transformed from an ideology of combat, in which political freedom, a secular state, anticorporatism, and economic development were advocated, into a unifying political myth in which Juárez emerged as the heroic leader of the liberal tradition and “constitutionalism,” an idea never fully realized under Porfirio Díaz, which became the revolutionary ideal of the opponents of the regime. The transformation of liberalism was facilitated in part by the ideas of a new generation of journalist-intellectuals who first enunciated a concept of scientific politics from the pages of *La Libertad* from 1878 to 1880. Influenced by the successful example of the conservative republics of France and Spain, they advocated a similar model of strong republican government for Mexico that would curb anarchy, restore order, and guarantee economic progress. Their technocratic orientation and interest in scientific politics was later reflected by the next generation of *políticos*, the *Científicos* of the National Liberal Union movement of 1892.

The *Científicos* sought constitutional reform that would limit the authority of the personalistic dictatorship of Díaz. By 1896, both positivism, established as the official curriculum of the National Preparatory School, and scientific politics were in the ascendancy and tended to reinforce one another. Yet, by 1903 it was obvious that personal government had supplanted establishment parties, including the National Liberal Union, and that the *Científicos* would have to sacrifice their program of constitutionalism and be-

come reconciled to military dictatorship. As for the "old" or classical liberals, they would have to express their liberalism outside of, and in opposition to, the establishment in the actions and writings of liberal critics such as Camilio Arriaga in 1901 or the radical *magonista* Partido Liberal Mexicano after 1905.

Hale has shaped his narrative in the best traditions of intellectual historical writing, paying attention to both internal history of ideas (analysis of ideas) and external history of ideas (the relationship of ideas to society and politics). The book is based almost entirely on published sources and includes the writings of politically oriented intellectuals (especially the *Obras completas* of Justo Sierra), public debates, ministry reports, and legislation. Newspapers are important sources for Hale, especially the scientific politics of *La Libertad* and the classical liberalism of *El Monitor republicano*. Hale's study chooses to ignore the orthodox positivist movement in Mexico, that is, the Pierre Laffitte branch of Comtean thought that found its expression in Mexico's Agustín Aragón and the *Revista positiva*. Perhaps the oversight is because of the lack of political impact of orthodox positivism in France or Mexico. In any case, Hale's study, a complete revision of earlier works by Leopoldo Zea, is the best and most complete work on liberalism in Mexico to appear from the desks of professional historians. It complements well his earlier studies on the intellectual life of nineteenth-century Mexico.

W. DIRK RAAT

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Fredonia

RICHARD GRAHAM. *Patronage and Politics in Nineteenth-Century Brazil*. Stanford: Stanford University Press. 1990. Pp. viii, 382. \$39.50.

Whatever the outcome of the current crisis of Latin American scholarship, here is one book that will not be swept into the dustbin of historiography. The great value of this study lies in its precise examination of local voting records in the reign of Pedro II (1840–89), in revealing for the first time the enormous reach of the electoral system and how it actually functioned. Richard Graham shows that for most of the period the polls "were open . . . to almost all free adult males, regardless of class or race" (p. 108). More than a million, at least half of the free male adult population, were registered to vote in the 1870s, and the great majority actually cast ballots. In the nineteenth century, Brazil's per capita participation in elections was among the highest in the world. Why did all of these people bother to vote—sometimes risking their lives in violent confrontations at the polling places—if, as generally acknowledged, the elections were rigged by the party in control of the cabinet in Rio de Janeiro, which never failed to win a parliamentary majority? Graham's answer is that the patronage system made them do it. Rural potentates

mobilized their dependent voters (sometimes gunmen) to win local elections and thereby position themselves to bid for the patronage of the officials in the imperial capital who determined the makeup of parliament. Those practices were facilitated by indirect elections prior to 1881 but persisted long after the changeover to direct elections.

Graham views Brazilian elections as "theater," as "dramatic acts" staged by the wealthy "to send a clear message to minor participants, identifying for them the grand persons to whom they owed deference and to whom they should render loyalty and obedience" (p. 269). Mass participation in these spectacles was encouraged until the specter of uncontrollable multitudes arose with the approach of the abolition of slavery. Stiffened registration requirements reduced voter turnouts to around one hundred fifty thousand nationwide in the 1880s. Graham assigns little social significance to the political changes of the 1880s, including the overthrow of the monarchy. He disputes the findings by class analysts, such as Caio Prado Júnior and Nelson Werneck Sodré, of basic conflict between urban and rural capital. The propertied classes who controlled government and society had intertwined economic interests, Graham maintains, and they shared a value system that placed a premium on "properly deferential behavior within a hierarchical social structure, [on] loyalty to one's patrons and care toward one's clients" (p. 5). A product of this system was the Brazilian bureaucratic state, which, Graham insists, remained subservient to those who dominated society, despite the contrary opinions of historians such as Raymundo Faoro.

This is a masterful study: imaginatively conceived, solidly researched, tightly reasoned, clearly and forcefully written. Graham's conclusions will be challenged, but his work will endure.

NEILL MACAULAY

Micanopy Historical Society
Micanopy, Florida

WILLIAM C. SMITH. *Authoritarianism and the Crisis of the Argentine Political Economy*. Stanford: Stanford University Press. 1989. Pp. xv, 395. \$42.50.

Political economy is once again in fashion among social scientists. William C. Smith's work is a sound and well-reasoned addition to recent books written by Susan Calvert and Peter Calvert and an edited volume by Guido DiTella and Rudiger Dornbusch. Smith, in his analysis of Argentina's dismal economic performance since the end of World War II, is aware that the economic dimension cannot be separated from the political. Nor can the often-bitter struggle between authoritarianism and democracy be divorced from economic policy and performance. He laudably rejects interpretations that would "reduce these complex events to an abstract economic logic" (p. 221).

Complexity, of course, creates its own set of prob-

lems but Smith deftly analyzes the bewildering number of factions and vested interests, each with their own agenda, that have rendered Argentina virtually ungovernable. The power of self-serving factions, among economic actors as well as in the military, labor, and political parties, is apparent in the Argentine fiasco. His treatment of the military during its self-styled "revolution" is particularly adept, and his exploration of the motives behind the policies of key figures, such as generals Juan Carlos Onganía and Alejandro Lanusse and economic minister Adalberto Krieger Vasena, is excellent. The *Cordobazo* of 1969 is correctly perceived as a spontaneous act, although it might not have been the watershed in recent Argentine history that Smith suggests. Indeed, the historical roots of many of Smith's themes run much deeper than is evident in his analysis. Although he appreciates the dual roles played by Juan Perón from his exile in Spain, the old leader seems too remote, too removed from the historical context in which he played key parts in the 1940s and 1950s. Similarly, Smith's analysis of organized labor and their goals and strategies is compelling for the recent period but lacks historical roots. Labor's pragmatism and cooptability dates at least to 1916. There is something else missing in Smith's analysis. The Calverts, in their

book on Argentine political culture and instability, and DiTella, in a chapter from his coedited work, suggest that, to appreciate fully the Argentine context, one must also treat sociocultural factors and the way in which they are reflected in economic and political performance.

Argentines have come to expect failure. This expectation translates into a lack of vision among economic and political leaders and a destructive self-centeredness as the various actors compete for perceived benefits, privilege, and power. Smith is aware of this dimension. "Inertial inflation," the belief that future inflation is simply a function of past inflation, is part of the problem. The memory of failure is built into the system. Smith also mentions, but does not analyze, the "famous Argentine *mufa*, that combination of collective frustration, desperation, and cynicism" that feeds a destructive privatism. On measure, however, this is a thoughtful work, an important work, and contributes significantly to our understanding of this complex period of Argentine history.

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Storrs

Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed.

C. T. MCINTIRE and MARVIN PERRY, editors. *Toynbee: Reappraisals*. Buffalo: University of Toronto Press. 1989. Pp. viii, 254. \$40.00.

C. T. MCINTIRE and MARVIN PERRY, Toynbee's Achievement. WILLIAM H. MCNEILL, Toynbee's Life and Thought: Some Unresolved Questions. CHRISTIAN B. PEPPER, Toynbee: An Historian's Conscience. C. T. MCINTIRE, Toynbee's Philosophy of History: His Christian Period. MARVIN PERRY, Toynbee and the Meaning of Athens and Jerusalem. THOMAS W. AFRICA, Toynbee: The Time Traveller. W. WARREN WAGAR, Toynbee as a Prophet of World Civilization. ROLAND N. STROMBERG, *A Study of History* and a World at War: Toynbee's Two Great Enterprises. BERNICE GLATZER ROSENTHAL, Toynbee's Interpretation of Russian History. EDWARD PESSEN, Toynbee on the United States. FREDERICK M. SCHWEITZER, Toynbee and Jewish History. THEODORE H. VON LAUE, Toynbee Amended and Updated. JANE CAPLAN, Working with Toynbee: A Personal Reminiscence.

JAN BREMMER, editor. *From Sappho to De Sade: Moments in the History of Sexuality*. New York: Routledge. 1989. Pp. x, 213. \$45.00.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editor's discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

ARTICLES

TO THE EDITOR:

I appreciated Steven F. Lawson's citation of one of my books [*AHR*, 96 (April 1991): 463], though the title was rendered inaccurately (cf. *Communist Front? The Civil Rights Congress, 1946–1956*).

However, your readers should know that the citation of works that reflect on the NAACP is incomplete in that it does not note my *Black and Red: W. E. B. Du Bois and the Afro-American Response to the Cold War, 1944–1963* (Albany, 1985). This may be the only work available that utilizes primary sources in detailing how the NAACP's response to the modern civil rights movement was shaped definitively by McCarthyism and the cold war. Lawson notes correctly that exploring how anticommunism and radicalism influenced the civil rights movement is the appropriate next stage of the historiography.

GERALD HORNE
*University of California,
Santa Barbara*

REVIEWS OF BOOKS

TO THE EDITOR:

This is the second successive occasion on which Robert Soucy has reviewed one of my books in your journal. Soucy would have to be a saint or an angel not to have seen it as a godsend. Let me explain: I did not react to Soucy's hostile review of the original 1983 French version of *Neither Right nor Left* (1986). After

all, no one has the right to expect that his work will always fall into the hands of friendly or open-minded reviewers. But now, since your journal has made a custom of asking for Soucy's opinion of my books, I must draw your readers' attention to the fact that Soucy has been given the golden opportunity to review two books in which he himself is very harshly treated. Having grasped this opportunity with both hands, it is doubtful, to say the least, whether he can lay claim to the minimum of objectivity normally required of scholars asked to inform the historical community about the contents (and quality) of a new book. In *Neither Right nor Left* (which, one should mention in passing, was chosen by *Le Monde's* literary supplement as one of the forty most significant books to be published in France in the 1980s), my reference to the work of Soucy (p. 307, n. 2) was by no means complimentary, and I can well understand that he was quite hurt by it. Is it an exaggeration to suppose that this is not unconnected with the character of his review in the *AHR* (90 [February 1985]: 148–49)?

Where the *Naissance de l'idéologie fasciste* is concerned (this book is now being translated into four languages, including English), is it not logical to ask once again if Soucy's review (*AHR*, 95 [December 1990]: 1527–28) was not influenced to some degree by my own verdict on his most recent book: "As for Robert Soucy's *Fascism in France, The First Wave*, it has so far attracted no attention, and rightly so" (*Naissance*, p. 401)?

With regard to the review itself, it needs to be said that Soucy has not understood or has not wished to understand the main ideas of my book. I do not even have the clear impression that he has really read it. All his references are taken from the beginning or the end of the book, without relating to the main body of the work, where I demonstrate my arguments in detail. This is no doubt why Soucy has understood neither the fundamental problematics of the antimaterialistic revision of Marxism nor the function of this revision in the inception and structuration of the fascist ideology.

Thus, when Soucy claims that on page 25 of my book I described the antimaterialistic revision of Marxism as a "synthesis" of Marxism and nationalism, that is not only erroneous but stupid. What one finds

on page 25 is a definition of fascism: "The second main component of fascism which together with anti-liberal and antibourgeois nationalism, made up the fascist ideology, was the antimaterialist revision of Marxism." Marxism and the antimaterialistic revision of Marxism are two totally different ideological systems. The antimaterialistic revision of Marxism meant a rejection both of Marxist economics and of the Marxist philosophy, which is to say of the rationalistic and Hegelian contents of Marxism. This whole book traces the development of this dual revision carried out by Georges Sorel and the Italian revolutionary syndicalists. Arturo Labriola and Enrico Leone were largely responsible for the economic revision, which resulted in their adoption of the most extreme liberal economic model, known in Italy as "liberism." These revolutionaries, who came from the most violently antibourgeois extreme left, were also those who most wholeheartedly accepted the rules of the game of the market economy. It was their economic—their purely economic—analysis that brought them to this conclusion. This, however, in no way altered their total opposition to the intellectual, philosophical, and moral content of liberalism, the Enlightenment, and the French Revolution. All they retained of Marxism was its dimension as a "sociology of violence." In this way, a new revolutionary path was opened up, disruptive of accepted habits and ideas. Soucy does not seem to have grasped this.

Here, one must draw attention to another factor that seems to have eluded Soucy's understanding, namely, that the revolutionary syndicalism that constituted the main intellectual equipment of Mussolini and the hard core of the founders of fascism, and that turned into national syndicalism, was undoubtedly a variety of socialism without being a variety of Marxism. In the same way, national socialism is also a form of socialism. This, once again, shows that Soucy understood nothing at all when he claimed in his review that I established "a generic bond between two such disparate ideologies as fascism and Marxism."

There was thus no contradiction between the fascist revolt against the intellectual, moral, and political values of liberalism and the fascist acceptance—derived from revolutionary syndicalism—of the market economy and of non-intervention by the state in the economy. For the revolutionary syndicalists, the national syndicalists, and the fascists, it was not a matter of defending the bourgeois order but of simply recognizing a reality as powerful as the laws of nature. Similarly, they saw no contradiction between a rejection of the economic and philosophical content of Marxism and an acceptance of Marxism as "a sociology of violence" and as a means to combat the moral and political values of liberalism.

ZEEV STERNHELL

Hebrew University of Jerusalem

ROBERT SOUCY REPLIES:

I would remind Zeev Sternhell that since 1966 I have repeatedly disagreed with a number of distinguished historians (none of whom has "hurt" me beforehand) for insisting that fascism, including French fascism, was fundamentally left-wing, "antibourgeois," or "revolutionary." These scholars include René Rémond, Raoul Girardet, Eugen Weber, Philippe Machefer, Pierre Milza, and Philippe Burrin. I have dealt no differently with Sternhell, who in 1980 wrote that "in France, real authentic fascism was always born on the left, never on the right" (in Stein Larsen, ed., *Who Were the Fascists?* [Bergen, 1980], 496). Since 1966, I have also presented a good deal of evidence in my own writings that I believe goes a long way toward refuting the fascism-as-leftism argument, at least where French fascism is concerned. This does not mean, of course, that I find nothing valuable in the works of the above-mentioned historians, which would be close-minded indeed, or that I find Sternhell's own work without considerable merit (see the last paragraph of my "unfriendly" review).

I find it somewhat curious to be lectured by Sternhell on the desirability of open-mindedness. However, if open-mindedness requires passive acceptance of an author's interpretation, despite massive evidence to the contrary (including some of the author's own evidence), then my review, I confess, was not open minded. Nor, in this respect at least, was it "friendly."

But was it accurate? In his letter, Sternhell states that it is "not only erroneous but stupid" to claim that he described fascism as a "synthesis" of Marxism and nationalism. On page 15 of his book, he writes, "fascist ideology is the product of a synthesis of organic nationalism and of the antimaterialistic revision of Marxism." To be sure, he emphasizes the antimaterialistic dimension of this revision and denies that fascism was a "variety" of Marxism. Nevertheless, he continues to insist that it was a "revision" of *Marxism* (not, say, of conservatism or reaction) and a "variety" of socialism, especially syndicalist socialism. My question is, where is the "socialism" in all of this—let alone Marxism (however "revised")? We are told that fascists came from "the most violently antibourgeois extreme left" (his letter), that fascism was "an unprecedented war machine against the bourgeois order" (pp. 45, 305, 317), and that this is why "so many" "men of the left" "slid toward fascism" (p. 45). Yet, elsewhere, Sternhell points out that, after fascists came to power in Italy, these same "men of the left," including Mussolini, became exceedingly forceful defenders of big business and big agriculture (pp. 45, 305, 317), i.e., of precisely that bourgeois order they allegedly abhorred.

So why call them "men of the left," particularly when thousands of genuine leftists were murdered or imprisoned by fascism in the 1920s and 1930s? In his

letter, Sternhell notes that all that fascists retained of Marxism was its "sociology of violence," and in his book he emphasizes that what they sought was not an economic but a cultural revolution. Fine, but why call it a revision of "Marxism" or a variety of "socialism" when a sociology of violence was espoused by right-wing thinkers as well and when the goals of fascism were never Marxist or socialist? As I pointed out in my review, according to Sternhell's definition of fascism, Friedrich Nietzsche, a fierce antisocialist, would qualify as a "man of the left" because he was culturally "antibourgeois." (For an insightful critique of the way Sternhell and others play the "definitional game," see William Irvine, "Fascism in France and the Strange Case of the Croix de Feu," *Journal of Modern History*, June 1991.)

My quarrel with Sternhell is not personal but methodological. It seems to me that by focusing on particular intellectuals rather than on mass movements, Sternhell's characterization of fascism is highly misleading—especially when some of the intellectuals he chooses as exemplary are hardly representative of mainstream fascism (Georges Sorel, for example).

As for Sternhell's *ad hominem* remarks, I would suggest that a much more effective way of discrediting me would be to do a review himself, perhaps even a substantive review, of my last book on French fascism—even if this means confronting a good deal of inconvenient evidence contradicting his own interpretation of fascism. Such a review, however "unfriendly," would surely do more to increase our understanding of fascism—which, after all, is far more important than either myself or Sternhell—than merely indulging in supercilious dismissals.

In this connection, it might be useful to readers of the *AHR* who are more concerned with the serious historiographical issues involved than with the degree of my saintliness or the limits of my intelligence (I am less optimistic about the former than about the latter) to know the exact words of Sternhell's "uncomplimentary" footnote on page 307 (actually p. 313) of his *Ni droite ni gauche*. This footnote criticizes me for "still not knowing the distinction between the Solidarité française and the Francistes on the one hand [and] the Jeunesses patriotes and the Croix de feu on the other." The unquestioned assumption here, part of the orthodoxy of the fascism-as-leftism school in France, is that while such minor formations as the Solidarité française and the Francistes may have been fascist, the much larger movements of the Jeunesses patriotes and the Croix de feu, which were socially and economically conservative, were not. For a good deal of evidence challenging this assumption, see my article "French Fascism and the Croix de Feu: A Dissenting Interpretation," *Journal of Contemporary History*, January 1991, as well as my book *French Fascism: The First Wave, 1924–1933*. I look forward to Sternhell's open-minded review of the latter.

ROBERT SOUCY
Oberlin College

TO THE EDITOR:

Throughout her review of my book, *The Ability to Mourn: Disillusionment and the Social Origins of Psychoanalysis*, Judith M. Hughes (*AHR*, 96 [February 1991]: 129–30) repeatedly misrepresents its contents. This objection is independent of any disagreements I may have with her evaluation of the book. She incorrectly states its principal subject and purpose, disregards the progressive nature of its argument, and fails to acknowledge its quantities of supporting evidence. The reader of her review must conclude that the book is weak in definition, disjointed in execution, and without scholarly grounding.

To begin her review, Hughes states that the book's goal is to reformulate a psychological approach to contemporary culture by undoing the total break with the Western past Sigmund Freud created with his theory of culture. This is *not* the purpose of my book. As is stated in its opening sentence: "The purpose of this book is threefold. First and foremost, it seeks to create a fresh understanding of the origins of psychoanalysis which is deeply social in character, emphasizing cultural values and symbols in the West which go back several centuries. Psychoanalysis arose, I think, as a creative response to the loss of these most meaningful constructions" (p. 1). To support her misreading, Hughes draws on one sentence that describes the purpose of Part 3 of this three-part book (pp. 8–9 of the introduction) and another sentence embedded in a detailed analysis of Paul Ricoeur's work on Freud (p. 305 of Part 3).

As the introduction goes on to make clear, my book is in three parts: psychological studies and dimensions of the origins of Freud's ideas (Part 1), social and historical studies and dimensions of his life, thought, and movement (Part 2), and cultural criticism of psychoanalysis (Part 3), which situates the findings and arguments of Parts 1 and 2 in the sweep of Western culture as a whole.

Disregarding its cumulative structure, Hughes begins her detailed discussion of the book with Part 2 and states that by "the social origins of psychoanalysis" I mean that Freud and others "underwent an introspective experience" involving "a sad giving up of the ideals of political liberalism," especially earlier attachments to religious symbols. This is not the explanation Part 2 offers. Rather, to explain the origins of psychoanalysis, I developed a theory of mourning, understood as a social and historical process, by drawing on the work of—among others—Melanie Klein (pining), D. W. Winnicott (disillusionment), and Max Weber (disenchantment). I then suggested that the mourning process can lead to social redefinition of self and from there to the creation of new systems of meaning (in this case, psychoanalytic thought). Although the word "mourning" is in the title of the book and is central to the book as a whole, Hughes does not once mention the

word. This is, I think, a spectacular instance of what one commonly calls "missing the point."

Turning to Part 3, Hughes declares that it is about Freud's cultural texts and how to read them. This is factually incorrect. I did summaries and close readings of the cultural texts at the end of Part 2. (Shaped as they were by the dynamics of the psychoanalytic movement, they had to be discussed in that context.) The burden of Part 3 is a critical discussion of Freud's theory of Western culture, building on the works of—among others—Philip Rieff, Paul Ricoeur, and Hans Blumenberg. Hughes fails to mention any of this.

Hughes concludes her review with a discussion of Part 1 and states, more or less correctly, what it is about. By this point, however, the reader can only conclude that my book is wholly lacking in continuity, since Hughes has described it out of sequence.

One final observation. Hughes mentions no scholarly sources in the book other than Freud, with the exception of Heinz Kohut, who was important only for Part 1, whereas the book builds up its new theory of the historical significance of psychoanalysis by first discussing the work of all the eminent Freud scholars and the related work of scholars in other fields, as I have suggested above. That is, each part has its own literature review. By this omission, Hughes "tells" her reader that my book's intellectual landscape is desert-like, devoid of scholarly water and life.

In a nutshell, I hope that readers interested in the themes and authors described in this letter will not be dissuaded by Hughes's incorrect portrayal from reading *The Ability to Mourn*.

PETER HOMANS
University of Chicago

Judith M. Hughes does not wish to reply.

THE EDITOR

TO THE EDITOR:

I feel obliged to respond to David Faure's review of my book, *Understanding Peasant China* [AHR, 96 (April 1991): 580]. The review takes an exceptionally hostile tone, while offering criticisms that are either unjustified or show a great misunderstanding of the book's purpose. The general thrust of Faure's review is the complaint that my book does not meet Faure's standards of historical scholarship—for example, it does not consult the Chinese or Japanese sources. I am, however, a philosopher, not a historian, and my book was never intended as a work of historical scholarship but rather as a philosophical discussion of problems of explanation and theory in the China field. I do not aim to provide a complete summary and assessment of any empirical problem in contemporary China studies. My purpose is rather to select certain important theoretical controversies within this field and discuss the methodological and logical problems that these controversies raise. The value of this exercise must be left to readers within the China field, but

Faure does me a disservice by suggesting that the goal of my book is somehow to exhaustively but inadequately survey the field and arrive at final conclusions about how things were in China.

Faure criticizes my selection of authors for scrutiny (surely a consideration properly within my own purview). Thus he wonders why my discussion of the problem of agricultural stagnation in late imperial China does not focus on Mark Elvin's work. Elvin is indeed discussed at numerous points in that chapter. I deliberately chose *not* to focus on Elvin, however, because his conception of the high-level equilibrium trap has already received so much critical discussion within the literature. Likewise, Faure suggests that Robert Marks's study of collective action in Haifeng County ought not to have been selected "because Marks's study is the weakest of three monographs . . . and the other two do not advocate a class conflict view of analysis." If Faure had read my chapter more carefully, he would have discovered that Marks was selected exactly because I was looking for a class-conflict theory of peasant rebellion in order to focus attention on the logic of such theories. My goal, once again, was not to decide the empirical question how things were in Haifeng County; it was to provide a useful discussion of one important theoretical framework that may be employed in attempting to understand peasant collective action.

Faure objects to my citing Chen Yung-fa (in a footnote) in support of the idea that the class-conflict theory of collective action fits twentieth-century Chinese movements better than it does nineteenth-century movements. But Chen describes his main results in just such terms: "The Party mobilized the majority of peasants to struggle against the old rural order by linking its well-publicized wartime programs to rural tensions between the two sides" (Chen Yung-fa, *Making Revolution* [Berkeley, 1986], 12). *Contra* Faure, Chen is quite insistent that the CCP maintained a consistent class-conflict program throughout its mobilization efforts in the Sino-Japanese war and derived a substantial part of its success precisely from its ability to mobilize and control such conflict.

On Faure's description of my position within the philosophy of science: my philosophy of science is not Carl Hempel's logical positivism, and I specifically cast doubt on the idea that explanation must always take the form of subsuming historical phenomena under general laws. Claude Levi-Strauss does not occur in my bibliography for the very good reason that his thought is irrelevant to the concerns of my book.

Finally, Faure gives the impression that my book offers an unfavorable assessment of G. William Skinner's important work on the macroregions of China. On the contrary, I discuss Skinner as one of the most creative and productive contributors to empirical China studies. It is difficult for me to imagine anyone reading the chapter devoted to Skinner's work and not apprehending the level of respect and appreciation I show for it. The most critical arguments in this

chapter are directed rather against the attempt by Barbara Sands and Ramon Myers to discredit Skinner's macroregions argument.

What I find surprising about Faure's review is the tone of malice that permeates it. Any author must be prepared to hear negative reviews of his work; Faure's review is, however, gratuitously offensive in tone and shows a surprising lack of attention to the substance of my argument. I can only interpret this review as an expression of territoriality and an unwillingness to contemplate a contribution to the China field by a philosopher.

DANIEL LITTLE
Harvard University

DAVID FAURE REPLIES:

I am sorry that Daniel Little is offended by my review of his book. Nothing in his letter has made me change my views. His allegations about my intentions are, of course, quite unfounded.

DAVID FAURE
University of Oxford

TO THE EDITOR:

Beverly A. Smith's review of my *Blood and Power: Organized Crime in Twentieth-Century America* quite misrepresents the book [AHR, 96 (April 1991): 625]. Smith says not a word about the book's two major themes: the ethnic patterns of Irish, Jewish, and Italian gangsters and WASP gangbusters, and the underworld code that used to protect honest lawmen. Thus Smith misses entirely my version of the assassination of John F. Kennedy. The mob—specifically, it appears, the Carlos Marcello family of New Orleans—killed Kennedy not just because of Robert Kennedy's crusade against the underworld. It was rather because the Kennedys were playing it both ways, accepting secret help from the mob in the 1960 election and in the CIA plot to kill Castro and then striking public postures against the underworld. This amounted to a double cross, a deadly violation of the underworld code, and resulted in a contract being put on the president.

Blood and Power argues against the Mafia-as-myth school of sociologists and historians, a group of scholars that flourished two decades ago and has had little new to say in the years since. Most academics now plowing these fields, however, went to graduate school in the 1960s and 1970s, and today they continue to doubt the existence of a national Mafia with a ruling Commission at its core. This position has been rendered obsolete by events of the last ten years, especially the scores of RICO cases brought against Mafia families around the country. As Smith suggests, no scholar should blindly trust what gangsters said in interviews, memoirs, or congressional testimony, when they were fashioning statements for a particular audience. The RICO trials have made available what

gangsters said in candor, when they thought nobody else was listening: thousands of pages of bugged and wiretapped conversations about the Mafia and its operations.

Today, the Mafia no longer controls hard drugs, and the Commission no longer meets every five years, as it did for decades. But no one with an open mind can read the RICO evidence and still doubt the realities of the Mafia. Like the U.S. Congress, the Mafia wields no absolute power, and many individuals defy its will. Everyone still agrees that Congress exists. The Mafia's rogue elements and new underworld competitors do not disprove the existence of the Mafia—especially when the Mafia kills them for their defiance.

Journalists and law enforcers have understood this situation for decades now. Academics drawing on outmoded authorities still have not heard the news. I can think of no starker dissonance between what some academics think they know in here and what is really going on out there.

STEPHEN FOX
Boston

BEVERLY SMITH REPLIES:

Contrary to Steven Fox's comments, my review does not deny the existence of organized crime. It does exist and at great cost to this nation. My review merely questions certain aspects of Fox's interpretation. RICO tapes, which Fox uses to advantage, are often recorded under circumstances that are less than ideal. Juries, as in recent John Gotti trials, have been reluctant to accept garbled taped evidence. In addition, organized crime figures are generally aware that their conversations may be recorded.

BEVERLY A. SMITH
Illinois State University,
Normal

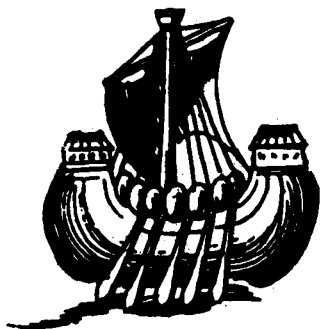
ERRATUM

In the review of Francis L. Broderick's *Progressivism at Risk: Electing a President in 1912* (AHR, 96 [April 1991]: 628–29), the following sentence appears: "Aided by progressives in Congress in passing the Fifteenth, Sixteenth, and Seventeenth Amendments, the Federal Reserve Act, the Federal Trade Commission Act, the Federal Farm Loan Act, and the Child Labor Act, Wilson gave progressivism an impetus that lasted over sixty years."

Professor Bernard Sinsheimer, Department of History, University of Maryland, European Division, reminded us that the Fifteenth Amendment was ratified in 1870 during the presidency of Ulysses S. Grant and that the Sixteenth and Seventeenth Amendments were proposed before the 1912 election, the Sixteenth ratified by February 25, 1913, while William Howard Taft was still president.

The editors apologize for not catching these errors.

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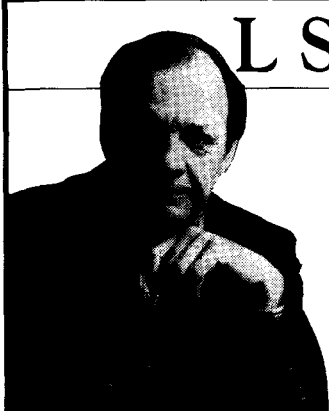
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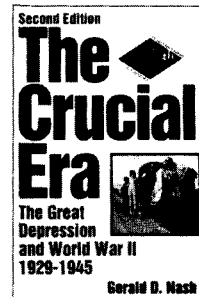
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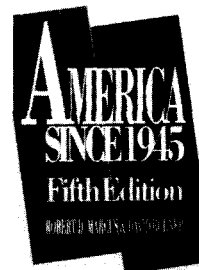


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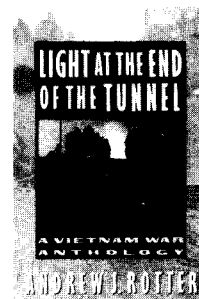


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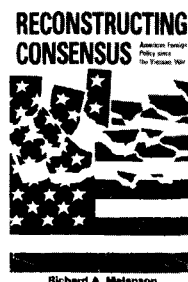
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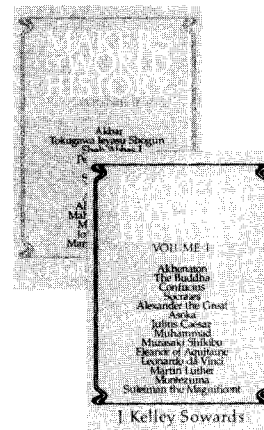
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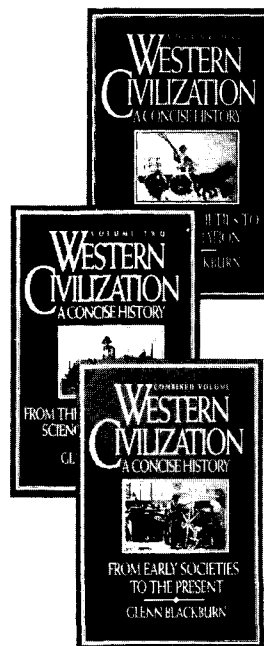
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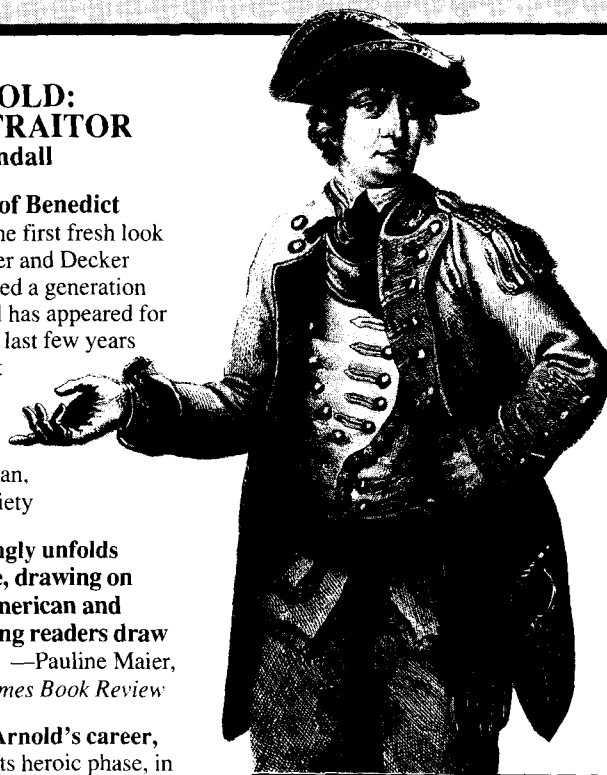
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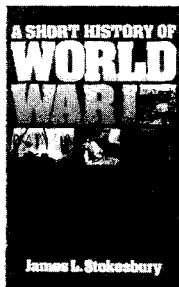
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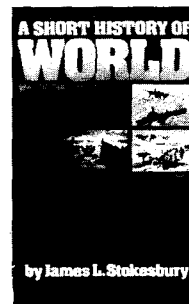
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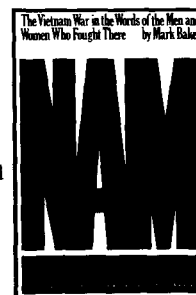
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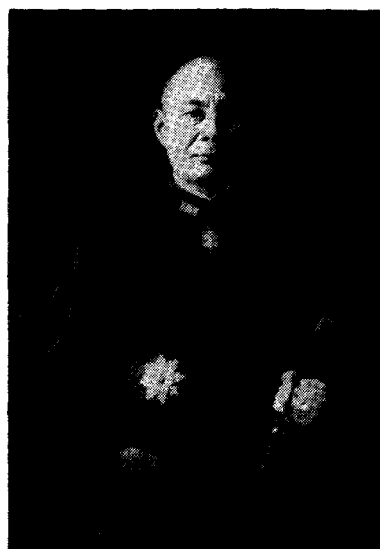
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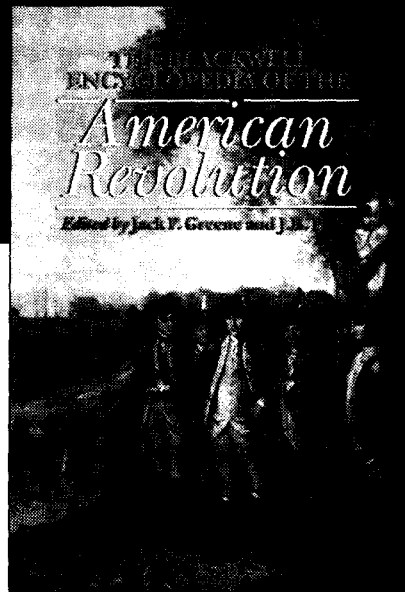
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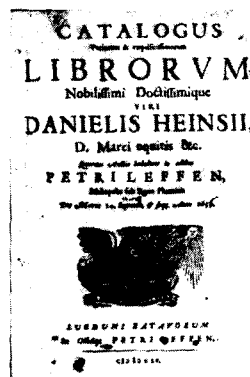
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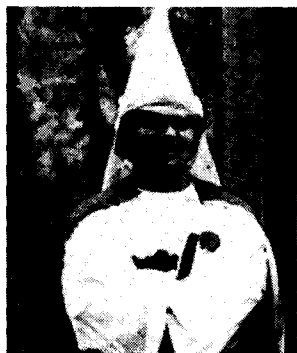
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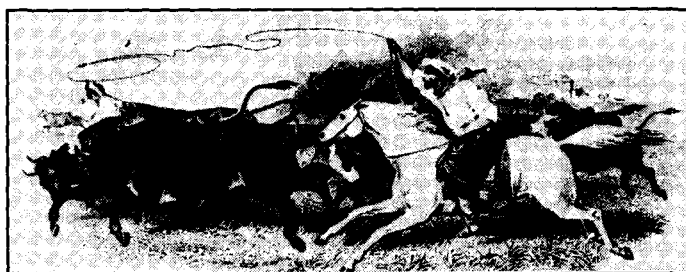
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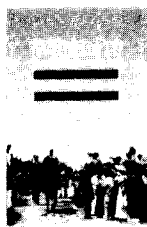


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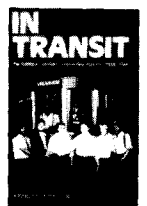
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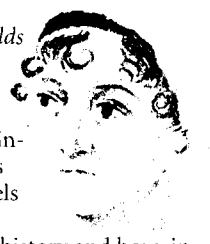
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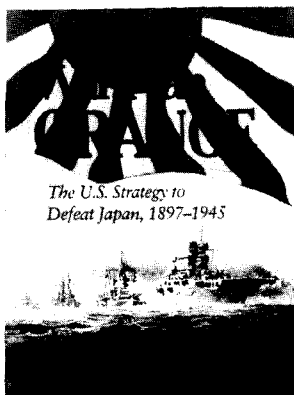
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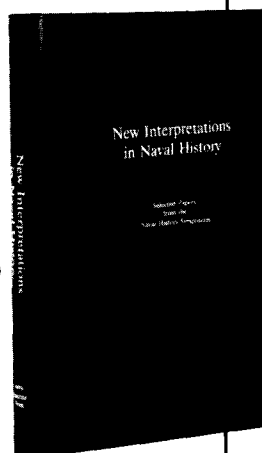
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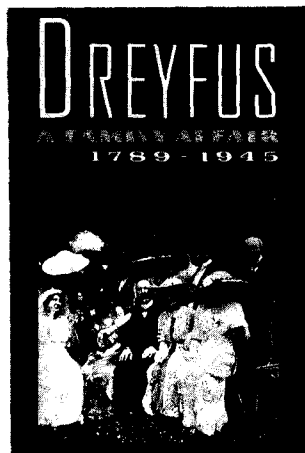
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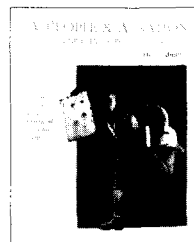
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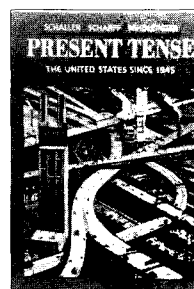
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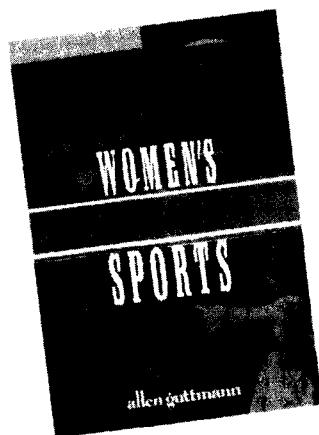
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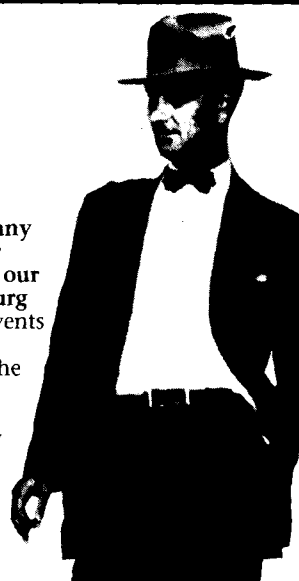
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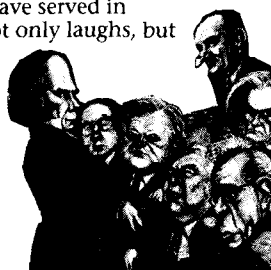
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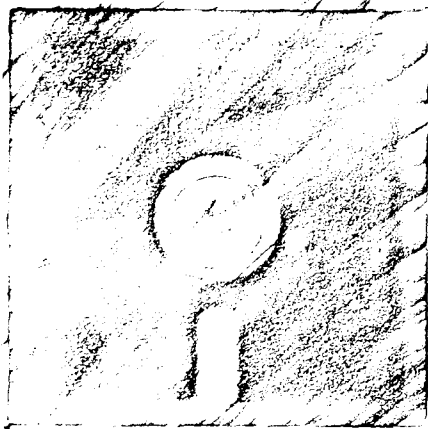
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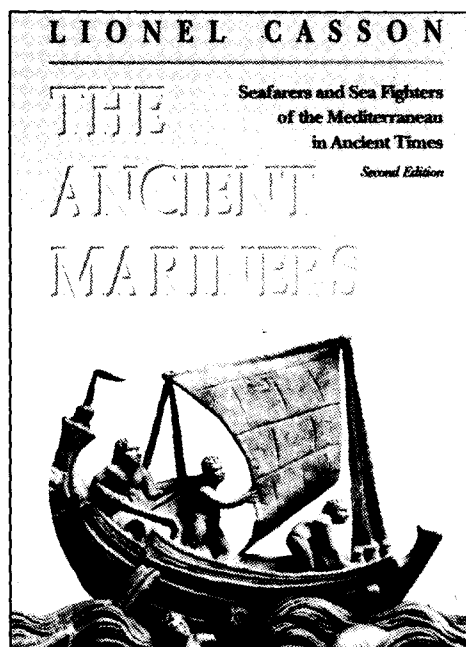
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